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ART. XXII.—*On a Chart of the Magnetic Declination in the United States, constructed, by* J. E. HILGARD, Assistant U. S. Coast and Geodetic Survey. With Plate V.

[From the United States Coast Survey Report for 1876.]

SIR: I submit to you herewith, for publication in the Coast Survey Report for 1876, a chart of the magnetic variation in the United States. This chart, which shows the lines of equal magnetic declination (so-called Isogonic lines) for the year 1875, is mainly based upon the observations made during the progress of the coast survey up to 1877, together with those made under my personal direction during the period 1872–'77, at the charge of the fund bequeathed for scientific research by the late Professor Alexander Dallas Bache, held in trust by the National Academy of Sciences.

When the income of this fund became available for its objects, I proposed, in 1871, to the board of direction, then consisting of Professors Joseph Henry, Louis Agassiz, and Benjamin Peirce, that a portion of it should be devoted to the investigation of terrestrial magnetism in the United States, that subject being one in which Professor Bache had taken much interest, and in the investigation of which he had been personally engaged. Moreover, while this was a subject of general importance, there was not at that time any provision made for its prosecution on the part of the government. The board of direction having approved of my proposition, an allotment was made for several years in succession, and the observations were prosecuted under my immediate direction by observers whom

I personally instructed in the work. In this way observations of the magnetic declination were made at about 200 stations, distributed over a large area of the interior country, at 150 of which stations the dip and horizontal intensity were also observed. These observations will be published in detail under the auspices of the National Academy of Sciences.

Subsequently, when on the extension of the scope of the Coast Survey so as to embrace the interior country, you proposed to undertake the requisite magnetic observations, the board of direction of the Bache fund deemed it best to close the work that I had been carrying on, and to publish the results obtained in the most available form, beside printing the observations themselves as a matter of record. Such publication can best be effected by combining them with all similar data available, and giving a graphic representation of the general result.

In the accompanying map this has been done for the declination (or variation of the compass) which is the element of the most practical utility. Since the data obtained by the Coast Survey form a very large part of the material used, an early publication in the Coast Survey Report is thought to be the most advantageous mode of giving the results to the country.

The incessant demands made upon the office of the Coast and Geodetic Survey for information relative to the variation of the compass in different parts of the United States bear evidence of the appreciation in which is held the similar map given in the Coast Survey Report for 1865 and published in 1867. The present map cannot fail to meet acceptably the constantly-increasing demand, as it is not only brought up to a more recent date, but is based upon a very much greater number of exact observations in the interior.

In its construction I have made use of all available data up to 1877, including, notably, beside the two principal sources already mentioned, the magnetic observations made in connection with the surveys of the Great Lakes and those of the Northern and of the Northwestern Boundaries by the United States Engineers, and those made under the direction of the General Land Office in tracing some of the principal meridians and base-lines for the surveys of the public lands and the boundaries of some of the Territories. Moreover, some very valuable observations have been furnished by private observers, which will be specified in another place.

I am indebted to Mr. A. Lindenkohl, chief draughtsman in the Coast and Geodetic Survey Office, for his valuable aid in the graphic construction of the Isogonic lines.

It was fortunate that, for the construction of this chart, the researches of my colleague, Assistant Charles A. Schott, on the

secular variation of the magnetic declination in the United States were available, without which it would have been difficult to reduce the observations to a common date, with some approach to accuracy. His latest paper on this subject, printed recently, will be found very useful for reference.

For a separate publication, it will probably be convenient to print Mr. Schott's map, illustrating the annual change, on the obverse side of the chart of magnetic declination, in order to make the sheet available for use without the aid of an explanatory text.

J. E. HILGARD,
Assistant Coast and Geodetic Survey.

To CARLILE P. PATTERSON, *Superintendent,*

U. S. Coast and Geodetic Survey Office, Washington, D. C., July 1, 1879.

ADDENDUM.—The approximate annual change of the declination for the epoch 1880 in different parts of the country is given below, as deduced from the map accompanying the valuable research on the secular variation of the magnetic declination in the United States, etc., by C. A. Schott. Appendix No. 8 to Coast Survey Report for 1874, third edition, 1879.

The observed amount of change is by no means the same even in places not far remote from each other, as New York and Philadelphia. In grouping together a table of the present rate of change much allowance must therefore be made for possible local peculiarities that have not been ascertained. For the interior States the information is very scanty, or altogether wanting.

The annual change is expressed in minutes of arc, a + sign indicating increase of westerly or decrease of easterly declination.

Locality.	Annual change.	Locality.	Annual change.
Maine, coast of	+2′	West Virginia	+3½
Maine, interior	+3	N. Carolina, S. Carolina, Georgia	+3½
New Hampshire	+3½	Florida, northern part	+3½
Vermont	+5½	Florida, southern part	+3½
Massachusetts, eastern part	+2½	Alabama, Missis., Gulf coast of	+3½
Massachusetts, western part	+3 to 4	Louisiana, eastern part	+3
Rhode Island and Connecticut	+3½	Louisiana, western coast	+2
New York, Long Island,	+2½	Texas, coast of	+2
N. Y., northern and west'n part	+4½	Texas, southwestern part	0
New Jersey	+3	Colorado	+2½
Pennsylvania	+3⅜	Utah	+ ½
Ohio	+2½	New Mexico and Arizona	0
Tennessee, eastern part	+2½	California, coast of	−1½
Tennessee, western part	+2	Oregon, coast of	−2 to 2½
Missouri	+2	Washington Territory, coast of	−2½ to 3
Delaware, Maryl'd, and Virginia	+3		

The negative sign indicates an increase of easterly declination.

J. E. H.

Art. XXIII.—*The Old River-beds of California;* by Joseph LeConte.

[Read before the National Academy of Sciences, Oct. 29, 1879.]

Old river-beds are found in nearly all countries which have been affected by drift-agencies. In nearly all such countries, too, these old beds are filled to great depths with river deposit. But the old river-beds of California are in several respects entirely unique. In most other countries, as for example, in Europe and Eastern United States, the new or present river-beds occupy the same position as the old; while in middle California the rivers have been *displaced*, by lava flows, from their former position and compelled to cut entirely new channels. Again: in certain portions of Europe and in Eastern United States, the old river-beds are broad, deep troughs, filled sometimes several hundred feet deep with detritus, into the upper parts of which the present much shrunken streams are cutting their narrower channels on a *higher* level; while in California the displaced rivers have cut their new channels 2000 to 3000 feet deep in solid slate, leaving the old detritus-filled channels far up on the dividing ridges. In northeastern United States the drainage system has remained substantially unchanged since early Tertiary, or even still earlier times; while in middle California the Tertiary drainage system seems to have been obliterated and the streams have been compelled to carve out to a much deeper level an entirely new and independent drainage system, having the same general direction but often cutting across the former. In the one case the old beds *underlie* the new, while in the other they *overlook* them from the tops of the neighboring ridges. Furthermore, in California the detritus which fills the old river-beds is nearly always capped with lava or other volcanic material, clearly indicating the cause of the displacement. If to all these peculiarities we add the usually extreme coarseness of the detritus which fills the river-beds of California, consisting as it does largely of pebbles and bowlders, compared with the fine silts which fill the old river channels of the Eastern coast, and we will see how marked is the contrast in many respects.

For all that is known concerning the old river-beds of California, we are up to the present time almost wholly indebted to Professor Whitney. His valuable investigations on this subject were published in the first volume of the Geological Survey of California in 1865. He has also recently published a fuller description and a complete map of them. His views have therefore been before the scientific public for many years, and are so well known that a bare enumeration of their main

features is all that is required here. Whitney shows: 1. that there is in California an old river-system entirely different from the present river-system; 2. that the old channels were filled by detritus, and the detritus covered by lava-streams; 3. that the lava flows, completing the filling of the channels, diverted the streams and forced them to cut for themselves new channels; 4. that the displaced streams cut their new channels to a much lower level than the old, so that these latter are now found on the present divides. My own observations entirely confirm these results, and they form therefore my starting point. Whitney also regards the old detritus as the representative of at least the whole Pliocene and the Lava flow as its closing event. In what respects my own views are an extension of Whitney's, and in what respects they differ from them, will be sufficiently indicated in what follows.

The general relation of the old and the new beds is well shown in the following figures taken from Geological Survey of California. In figure 1 the old and new beds are parallel, and the section is across both; while in figure 2 the new beds have cut across the old, and the section is along the old and across the new.

This peculiar relation of the old to the new river-beds does not characterize the whole Pacific slope, but only the auriferous slate belt of middle California. It is not found in the Coast Range, nor in the region of the granite axis of the

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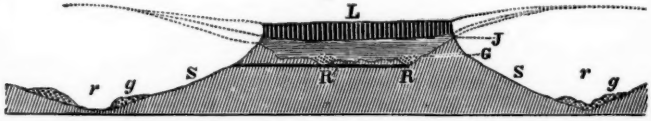

FIG. 1.—Section across Table mountain: *L*, lava; *SS*, sandstone;* *G*, old gravel; river-bed; *S*, slate bed rock; *r*, present river; *g*, present gravel.

2.

FIG. 2.—Lava stream cut through by modern rivers: *aa*, basalt; *bb*, volcanic ashes; *cc*, Tertiary; *dd*, Cretaceous; *RR*, direction of old river-bed; *rr*, of new river-bed.

Sierra Range. Neither is it found, at least in any marked degree, in extreme northern California nor in Oregon, nor yet in southern California. It seems to be confined mainly to the slate belt of the western slope of the Sierra from Plumas

* The material here called sandstone is a cemented river sand. It is usually covered with tufa or tufaceous conglomerate and this latter with lava.

county on the north to Tuolumne county on the south inclusive, a distance of about 250 miles, and from the San Joaquin and Sacramento plains on the west to about 4000 feet elevation on the Sierra slope on the east, a breadth of about 35 miles.

There is no problem in California geology more important, and yet none more difficult—none more enticing and yet none more baffling—than the mode of filling of the old river-beds and the cause of the displacement of the streams. The opportunities of study are abundant, for in many places the old beds have been bared, and complete sections of their fillings made by the operations of hydraulic mining; but the phenomena are so extremely complex and difficult of interpretation that we are not yet prepared for a final theory. I have on several occasions utilized my vacations by the study of these old channels and their fillings. In 1877 I examined those about Forest Hill; in 1878 those in Tuolumne county. Very recently under the intelligent guidance and kind assistance of Mr. Hughes, the superintendent of the Blue Tent mines, I have made a more extended and thorough examination than ever before. Extensive gravel deposits exist on both sides of the South Yuba River for many miles. This is in fact the finest mining region in the State. I went up one side and down the other and examined these in succession. I wish now to very briefly present the conclusions to which I have provisionally come by much reflection on the observations made on this and on previous occasions. I present them with some misgivings, well knowing that much more complete and detailed observations are necessary before an entirely satisfactory theory can be reached.

General Description.

It is well known that in hydraulic mining the whole thickness of the old river-channel-fillings is worked down and carried away by the prodigious force of the hydraulic jets. In such a mine, therefore, we always have the old bed bared over the whole area cleared, and the fillings exposed from bottom to top, on the face of an ever-receding vertical cliff 200 to 400 feet high.

The Bed.—The old stream-bed thus exposed has a shallow, trough-like form, i. e. is lowest in the middle and rises gently on both sides. These higher sides of the trough are called the "*rims.*" The *bed-rock*, which is usually slate with nearly vertical cleavage, retains usually its original soundness and hardness, but in some places is more or less decomposed, and sometimes, while retaining its form, is completely changed into plastic clay. In all cases it is worn into irregular and fantastic hollows and channels, and often into deep pot-holes. As there is no apparent relation between the hardness or softness of the

bed-rock and the amount of wear, it is certain that the softening has taken place since the filling. The surface-forms of the bed-rock are precisely such as are always produced by swift currents carrying coarse materials—precisely such as are *now* produced in the artificial channels through the rim-rock by the rushing torrents loaded with pebbles and gravel, resulting from the incessant play of the hydraulic jets against the cliff.* There can be no reasonable doubt, therefore, that these trough-like depressions are really the old channels of rushing torrents loaded with eroding materials.

The general form of the wide, shallow, trough-shaped channels of the old rivers is in marked contrast with the deep, sharply V-shaped cañons which characterize the present rivers in the same region.

The filling.—The cliff exposed by hydraulic mining, consists usually from bottom to near the top, of distinctly but irregularly stratified material. The lowest portion next the bed-rock, sometimes a few feet, sometimes many feet in thickness, is a conglomerate of pebbles and bowlders often of large size, with a paste of sand and plastic clay usually of a slate-blue color. This is the "*Blue gravel*" of the miners. The pebbles and bowlders are usually well rounded ("wash gravel"), but in a few channels I have found them sub-angular, like those of *till*. Above the Blue gravel, the whole way up to near the top, the material consists of alternate layers of pebbles, gravel, sand and clay, usually of a yellowish or reddish color. The pebble layers occur in lenticular masses, and the sands and clays are often cross-laminated. In many cases the whole material is more or less firmly cemented by lime carbonate or by silica, so that the cliff must be loosened by blasting before it can be washed down by the hydraulic jets. Irregularly distributed throughout the whole mass are found fragments or sometimes large trunks of drift timber, oak, maple and conifers, in a lignitized or else in a silicified condition. In some cases the lignitizing change has progressed but a little way. I found at Sailor's flat, beneath the volcanic cap, to be presently described, logs of Redwood (Sequoia) or of cedar (*Libocedrus*) probably the latter, in which the bark was still tough and fibrous although the wood was soft and could be cut like cheese. In the finely stratified sands and clays are found beautiful impressions of leaves of many kinds. According to Lesquereux these leaves indicate a Pliocene age for the deposits. More rarely mammalian bones have been found. Among these are allies of the rhinoceros, hippopotamus and camel, indicating, like the leaves a Pliocene age, but also in many undoubted cases the mam-

* It is well to note here the prodigious rapidity of this erosion. In the North Bloomfield mine, the pebble-loaded torrent, working eight months per year, has cut in four years a channel three feet wide and fifty feet deep in solid slate.

moth, the great mastodon and a tapir, undistinguishable from the living species, indicating a Quaternary rather than a Pliocene age. These Quaternary remains have been in several instances found under the volcanic caps in the lowest blue-gravel, next to the bed-rock. Several examples of this kind are now in the museum of the University. Some human remains and implements are also supposed to have been found in this detritus, but the authenticity of these is disputed by many.

In several cases I observed in the vertical cliff of detritus distinct curved lines of discontinuity, concave upward, indicating sub-channels cut in the main mass of detritus. Undoubtedly the main channel had been first filled, then partly swept out by erosion, and then re-filled. This observation is important, as it seems demonstrative of a true river agency.

The lower portion of the detritus, the so-called blue gravel, differs from the upper portion partly in structure, but chiefly in color. In structure it is almost if not quite devoid of lamination; and when the rock fragments are sub-angular it is almost undistinguishable from true *till* or ground-moraine. In most cases, however, its pebbles and bowlders are perfectly rounded. Its blue color is undoubtedly due to the fact that its iron is in the form of ferrous instead of ferric oxide. There is no such line of demarkation between the blue and the red gravel as would indicate a different origin. On the contrary the irregularity of the plane of contact and the shading of the color shows a downward progressive oxidation of iron, greater in some places than in others.

The capping.—Above the detritus which constitutes the main portion of the filling of most of the old river-beds, we nearly always find a capping of volcanic matter 50 to 150 feet thick. This is sometimes hard basalt underlaid by tufaceous conglomerate, but more usually tufaceous conglomerate only.

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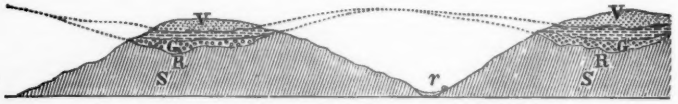


FIG. 3.—*SS*, slate bed rock; *RR*, old river bed; *r*, present river bed; *GG*, old river gravel; *VV*, volcanic conglomerate.

In this latter case, however, the presence of scattered blocks of basalt on the surface often indicates the *former* existence of a thin basaltic cap which has been removed by erosion. When a basaltic cap remains it gives rise to flat table-topped ridges as in figures 1 and 2. Otherwise the ridges become rounded by erosion. Figure 3 is an ideal section showing the more usual form. When the cap consists of tufaceous conglomerate above, the whole thickness of the filling, including the

volcanic cap, may be washed down together by hydraulic process, although the latter is of course barren matter; but when the cap is basaltic the auriferous gravel can only be worked by the slower process of drifting. In this case only the lower portion is extracted. In all cases the volcanic cap, especially the tufa, has furnished down-percolating, alkaline waters holding silica in solution. The condition of greater or less saturation (with perhaps other conditions)* seems to have determined whether this solution deposits or takes up silica. When it deposits, the gravel is cemented and the drift-wood is petrified; when it takes up silica the volcanic and slate pebbles are rotted down to "*putty stones*" and the bed rock is softened to a greater or less depth.†

The tufaceous conglomerate which is so constant an attendant of the old river gravels consists of a soft, earthy, reddish tufa, enclosing rounded pebbles of all kinds, volcanic, slate and quartz and of all sizes, distributed more or less abundantly but irregularly through its mass. It is probably volcanic ashes washed down from the higher Sierra slope by rain and melting snow. But the absolute absence of the least trace of stratification (which I sought for in vain) seems to show that the quantity of ash in proportion to water was so great that it was literally a mud-flow gathering pebbles in its course. In some cases, however, are found alternate layers of gravel and tufa. Where the basaltic lava occurs it always nearly overlies the tufa.

Nearly all the higher parts of the country are covered both with the gravel and with the tufa. The lava is also very widely spread. The indications are that these materials formed at one time an almost universal covering, but subsequent erosion has left them in ridges and patches.

Explanation of the phenomena above described.

There are many difficult and important questions suggested by the phenomena above described which press upon us for solution. "How were the old river beds filled with detritus?" "How were the streams displaced from their old beds?" "Why have the new channels been cut so much deeper than the old?" "When did these events occur?" I shall take these four points in the order mentioned.

1. *The mode of filling of the old river-beds.*—There are three possible modes in which we may conceive these beds to have

* In silicification of wood there is little doubt that the percolating alkaline waters charged with silica are neutralized by the acids of organic decomposition, and the silica thus rendered insoluble is deposited then and there (see author's Elements of Geology, p. 193.)

† In many of the hydraulic mines, especially in the Dardanelle mines near Forest Hill, I found in certain parts all the slate and volcanic pebbles while still retaining their form perfectly, reduced to a soft soapy bluish clay. These are called putty stones. It is evident their silica has been extracted by alkaline waters.

been filled with detritus. 1st. They may have been filled by glaciers slowly and steadily retreating up the valleys, dropping their debris on their way, the debris perhaps afterwards modified by currents from the melting glacier. 2d. They may have been filled successively from mountain foot toward mountain crest with detritus brought down by the rivers into a bay or fiord which steadily moved up the valley by subsidence of the land until the sea stood 4,000 feet above its present level. Or 3d, they may have been filled in all parts nearly simultaneously by true river action in the same way as river-channels elsewhere have been or under certain conditions are now filled.

I shall not discuss the first and second. I mention them because each, but especially the second, has been held by some persons. I have kept them constantly in mind during all my observations but have been compelled to abandon them as untenable. I am quite sure that no one can examine these deposits carefully without being convinced that they are true river deposits, though formed, certainly, under very exceptional conditions. It is these conditions which I now wish if possible to realize in imagination.

That the conditions were really exceptional is very evident on a little reflection. The present rivers in all this region run with high velocity, have cut very deep channels and are still cutting: why then should the former rivers in the same region have *filled* up instead of cutting their channels? The difficulty is not removed by supposing a lower velocity; for the character of the deposits, especially the great bowlders, often many tons weight, show a much higher velocity than now exists. With such rushing torrents why did the beds fill up instead of cutting deeper? To this I answer: any current, however swift, will deposit if *only its load be sufficient.* Every current has a certain amount of energy, and can therefore do a certain amount of work, increasing of course with the velocity.* This energy is usually divided between the work of transportation and the work of erosion. If the load of transported matter be moderate, a large amount of energy is left over for erosion; but if the load of transported matter be very great, the whole energy may be expended in transportation and none is left for erosion—the limit is reached at which erosion ceases and deposit commences. Now, since transported detritus is the main erosive agent, it follows that in every stream there is a certain amount of detritus which produces the maximum erosion. Pure water has little effect for want of erosive agent; too much material also produces little effect, because too much energy is consumed in carrying.

It is evident therefore that all that is necessary to cause any stream to deposit is to increase its load beyond the

* Gilbert, this Journ., vol. xii, pp. 16 and 85, 1876.

limits of its energy. This principle is well understood by hydraulic miners. The amount of water gathered in the sluices from the hydraulic jets must be duly related to the amount of earth removed. If the water is in excess, the precious water is wasted and the erosion of the sluices is very great; if the earth is in excess the sluice is choked, even though the velocity under proper conditions is sufficient to carry bowlders of several cubic feet. The water must be *well-loaded* but not *over-loaded.* The same important principle is well illustrated by the phenomena of the floods of the tributaries of the Sacramento River. As I learn from my nephew, Julian LeConte, who has been engaged in the hydrographical survey of this river, at the time of flood, the rushing waters first come down Feather River bringing only fine silt and clay; the water rises and increases proportionally in depth. Next comes the great mass of coarse sediment, sand, gravel and pebbles creeping slowly along the bottom and filling up the bed twenty feet deep; the water though in full flood is but little deeper (though much wider) than before the flood.* Lastly, as the water falls and has less sediment to carry, it again takes up the sediment previously deposited and scours out the channel even though its general velocity is now far less than when the same was deposited. In this case the filling is not permanent; but cases are not wanting of steady building up by rivers of very high velocity. According to the authority already mentioned the Yuba River at Marysville has permanently filled up its beds 30 feet deep, and 15 miles above Marysville 115 feet deep, in the last 30 years. This is wholly due to the large increase of transported matter produced by the operations of hydraulic mining. Again, according to Captain Dutton,† the Colorado River through its cañon and the Platte River over the plains have about the same slope, viz: eight feet per mile; but while the Colorado has cut its wonderful cañon and is still cutting, the Platte has filled up its channel and is still filling. The sole difference is the amount of load carried; the Colorado is *under*loaded, the Platte *over*loaded.

It is evident therefore that river deposits cannot, like ocean and lake deposits, be taken as a measure of time. Rivers either erode or build up by deposit. If they build, they almost always build very rapidly; for the carrying power of running water varies at so high a rate that a very slight change in conditions affects enormously the amount of deposit. While they build, therefore, they build rapidly but are liable under even very slight change of velocity or amount of sediment to scour out again. For example, Feather River fills up 20 feet in a single flood and scours out again when the flood subsides.

* The rise of the surface is about 23 feet; the filling of the bed 20 feet.

† Nature, vol. xix, p. 274, 1879.

But if the overloaded condition be permanent or habitual, then the building is permanent as well as rapid. For example, the Yuba River above Marysville has built up 115 feet in 30 years. I believe that most thick river deposits, whether of the present or of previous epochs, have been made in comparatively short space of time.

Now the phenomena of the old river-gravels, as I have described them, are precisely those of deposits made by the turbulent action of very swift, shifting, overloaded currents; only in this case the currents must have been far swifter and more heavily loaded than any existing currents. The detritus of the old river beds is usually exceptionally coarse; therefore the rivers at the time of deposit must have been exceptionally rapid; and therefore also the quantity of material necessary to overload them must have been exceptionally great; and therefore finally the process of filling was probably exceptionally rapid. It might have occupied years, or even centuries, but was geologically a very rapid process. Now I cannot conceive how all these conditions could have been fulfilled, except by the rapid melting of extensive fields of ice or snow. But why —it will be asked—was the detritus not carried away again? I answer: Because immediately after the filling was completed the detritus was protected and the rivers displaced by the lava-flood. This brings me to the next question, viz:

2. *The cause of the displacement of the rivers.*—As already shown, the mere filling up of the river channels with detritus alone would never have displaced the streams. On the contrary, as soon as the conditions determining the filling were changed, the rivers would immediately have commenced cutting into the detritus as they have done on the Eastern coast; and, on account of the high slope of their channels, would ere this have completely swept it all out, as they have done in Southern California. The protection of the detritus and the displacement of the streams is due wholly to the lava flood.

Middle California lies on the southern skirt of the great lava-flood of the Northwest.* The center of the great outflow was the Cascade and Blue Mountains. In Oregon the lava is 3,000 feet thick and therefore completely conceals the previous surface configuration of the country. In extreme northern California it is still a universal mantle several hundred feet thick, and therefore the old river beds with few exceptions are hopelessly concealed. In Middle California we find it reduced to ridges and patches by erosion, but originally it probably was even here a nearly universal mantle, covering the whole surface, except some highest points, and substantially obliterating the drainage system. But yet this lava mantle was not so

*See article by the writer on this subject in this Journal, vol. vii, p. 167, and p. 279, 1874.

thick but that subsequent erosion has cut through the *thinner* parts, i. e., on the previous higher ground. Immediately after the obliteration of the previous drainage system, the rivers, of course, commenced cutting a new system, having the same general trend (for this is determined by the general mountain slope), but wholly independent of, and therefore often cutting across, the older system. Furthermore the streams in forming their new beds seem to avoid the places of the old beds, for there the lava would be thickest, and cut their channels on the old divides for there the lava was thinnest and therefore soonest removed by general erosion, or perhaps was absent altogether.

Again: we have already seen that the rush of overloaded waters which filled the old river-beds with detritus, could have been produced only by rapid melting of snow and ice. We have seen also that the process of filling must have been comparatively rapid. Still further we have seen that the detritus must have been quickly protected and the streams diverted by the lava-flow. Bearing these things in mind we are naturally led, nay we are almost driven, to the conclusion that the approach of the subterranean heat of the impending lava-flow was the cause of the rapid melting of the snow and the consequent rush of the overloaded waters which filled the channels with detritus. Before the melting was completed the ash-eruptions had already commenced, and mud-streams, followed by lava-streams, completed the work of obliteration. We see precisely the same phenomena on a *small scale*, in the destructive floods and mud-streams which precede and accompany the eruptions of volcanos like Cotopaxi, whose summits are capped with perpetual snow. In the case we are discussing, however, instead of a volcanic peak, a great mountain range covered with snow, erupted.

Some geologists of the Uniformitarian school, may object to the foregoing views, as savoring too much of Catastrophism. In answer I would remark that it is simply impossible to account for wholesale obliteration of a river system except by something like a catastrophe. Powell and Dutton* have shown that of all geographical features, river courses in elevated regions are the most permanent. In early Tertiary times the Green River was winding its devious course southward when the Uinta Mountains commenced to rise directly athwart its pathway; but the river maintained its level and its course by cutting downward in proportion as the mountain rose upward. Farther south the Colorado plateau commenced to rise; but the river still maintained its level and its course by cutting downward in the same proportion. When once a river, as it were, bites in and gets a grip upon the rocky bones of the

* Powell, Exploration of Colorado river, p. 152; Dutton, Nature, vol. xix, p. 247 and 272, 1879.

country it does not easily loose its hold. Rivers with deep channels like those of California will not change. Their channels must be obliterated, and then they make new channels. Such obliteration can only take place by submergence and prolonged sedimentation or else by a lava-flood.

We have seen that tufaceous conglomerate usually underlies the basaltic lava and covers the detritus even where the lava is wanting. It is evident therefore ash eruptions preceded the basaltic flow. The washing down of these ashes as mud streams completed the filling and then the lava flood covering all prevented the re-cutting of the channels in the same places. Furthermore, if we imagine the ash flood as even more general than the lava flood, it is easy to see how the new channels would commence between the lava streams, i. e., between the old stream beds, and once commenced would continue to cut in these places.

King* has drawn attention to the fact that in the same locality and therefore presumably from the same subterranean igneous reservoir *acid* eruptions immediately precede *basic* eruptions. He accounts for this order by supposing a gradual separation, by gravity from the same fused magma, of a lighter, acid, less fusible portion as a sort of scum on the surface of a denser, basic, more fusible portion. Eruption would of course commence with the ejection of the upper, acid, and finish with the lower, basic, portion. The eruptions of which we have been speaking seem to confirm this idea. The imperfectly fused, or aqueo-igneously fused, upper and more acid portions were ejected first as *ashes* and only later the igneously fused, basic, bottom portions were ejected as basaltic flows.

The conditions necessary to produce the double system of river-channels are peculiar and found only in the Sierra range of Middle and Northern California. In extreme Northern California and especially in Oregon the lava flood is so thick that the buried old river system is not revealed by erosion—the present rivers are running far above the old rivers. In Southern California on the contrary the rivers have never been displaced by lava, for the lava flood did not reach so far. If these channels were ever filled with detritus, this has not only been swept out again, but the rivers have continued to deepen their channels even to the present time. The double river system of Middle California is the result of the fact that this part lay in the extreme skirts of the great lava flood. In British Columbia beyond the limits of the lava flood, the relation of the new to the old river beds, as I learn from Mr. Amos Bowman, is again like those of the Eastern States. The rivers are now cutting into the detritus which fills the broader and deeper channels of the old rivers.

* Exploration of 40th Parallel, vol. i, Systematic Geology, p. 715.

3. *Why the modern rivers have cut to a lower level.*—I have already in a previous article* given reason to believe that the great lava flood of the Northwest came not from craters but from great fissures, and that the force of eruption was not the pressure of elastic gases merely, but also the lateral squeezing by which mountain ranges are elevated. It is almost certain, then, that coincident with the outflow of lava in California there was an increase in the elevation of the Sierra range. The inevitable effect of this would be the cutting of the new channels below the level of the old, and thus finally the singular relation between the old and the new channels which now exists.

There is a certain definite relation between the slope and the amount of detritus which determines the depth of the cañons. If this relation be disturbed by increase of slope, the stream will strive to reëstablish it. All deep cañons have been cut in rising ground and for the purpose of reëstablishing this relation. Thus it has been with the great cañons of the plateau region; thus also with the cañons which trench the eastern slope of the Colorado Mountains; and thus it must have been with the cañons of the Sierra Nevada. Again there is a certain relation little understood, between rain-erosion and stream-erosion. In Tertiary times we may imagine the conditions were favorable for general rain-erosion and unfavorable for stream-erosion or cañon cutting; and the result was a system of broad, shallow, trough-shaped channels with low divides. Since glacial times, on the contrary, the conditions have been favorable here for cañon cutting. Among these conditions the slope is certainly most important. It is difficult to imagine that the Tertiary river channels should have remained so shallow after the erosion of the whole Cretaceous and Tertiary times, if the general Sierra slope were as high *then* as it is *now*, viz: 100 to 200 feet per mile. It is true that the great glaciers of glacial times have probably greatly assisted in cutting the present cañons; but this would only affect the amount of time required, not the final result; for if glaciers cut deeper than streams would have done, these streams would again fill up their channels until the proper relation was again established.

The elevation which I suppose took place in the Sierra range at the time of the lava flow, was evidently of a gentle kind, unaccompanied with crumplings and dislocations of the strata, and therefore undetectable except by the work of cañon-cutting. The axis of the Quaternary elevation on the eastern portion of the continent was probably along the valley of Mississippi River; the axis on this side was the crest of the Sierra, where it gave rise to fracture and outflow.

The places where the lava emerged have not been found with certainty. It was probably along or near the crest, where the

* This Journal, vol. vii, p. 177, 1874.

subsequent erosion has been so great that the evidences are mostly obliterated. In Alpine County, about Silver Mountain and about Markleeville, the Sierra crest is formed largely of volcanic rocks. From this region probably a large number of streams radiated. But over the larger portion of the high Sierra erosion has bitten so deep that the lava streams have been entirely removed. I have however observed many dioritic, doleritic and felsitic dikes in all the granite region above the lava flow. These I have thought are probably the exposed roots of the flow.

4. *The age of the old river gravels and of the lava flow.*—Whitney and Lesquereux, on the evidence of the organisms, especially the plants, refer the gravels to the Pliocene, and regard them as representing at least the whole of that epoch, and the lava flood as its closing event. My own conclusion differs a little from this. I have already shown that the accumulation of the gravels and their protection by the lava flow may be regarded as geologically almost simultaneous. I now add that these two events closed the Pliocene and inaugurated the Quaternary.

As already seen, the *mammalian* remains are a mixture of the characteristic Pliocene species still lingering and of characteristic Quaternary species just coming in. They undoubtedly therefore indicate a transition from Pliocene to Quaternary, and whether on these evidences we refer the gravels to late Pliocene or early Quaternary will depend upon whether we regard as the more important test of age, the extinction of old or the introduction of new species. The evidence from human remains and implements, if these be regarded as authentic, is certainly on the side of greater recency. The *Plants*, it may be admitted, are Pliocene. But plants are far less delicate tests of age than mammalian animals: for not only are they, by their lower organization, less sensitive to changes of the environment; but being incapable of voluntary migrations, they are often compelled to linger beyond the epoch to which they belong. It is natural to suppose, therefore, that the Pliocene flora would linger even into the Quaternary until destroyed by the extreme rigor of glacial climate, or else by some catastrophe like that of the lava flood.

Again: the general phenomena of the gravels and the manner of their accumulation, as I have explained them, are wholly those of the Quaternary period. They can hardly be explained except by the existence of glacial conditions. Also the gentle movement of elevation which we have supposed preceded and attended the lava flow is characteristic of the Quaternary everywhere. It is probable, therefore, that the gradual elevation and the attendant glacial conditions commenced and advanced until the former culminated in fracture and outflow of lava.

But on the other hand, it is certain that the Pliocene passed insensibly into the glacial epoch, and therefore that glacial conditions commenced in the Pliocene. Furthermore, it is certain that here in California glacial conditions continued and reached their acme *after the lava flow;* for glaciers occupied all the present cañons,* and *swept away all the lavas* from the granite axial region, exposing their roots, in the form of dikes.

In conclusion, therefore, it seems best to make both the accumulation of the gravels and the lava flow which protected them, the dividing line between the Pliocene and the Quaternary, although I believe that glacial conditions had already commenced when these events occurred.

In a previous article already referred to, I have shown that the great lava flood commenced in its central part in Oregon, about the beginning of the Pliocene epoch, and has continued there almost to the present time. But as in volcanos the eruptions commence and perhaps continue in a central crater, and as erupted matters accumulate, later eruptions occur also on the outer margins; so in this great area of fissure eruptions, the eruptive activity commenced first in the center, but as erupted matters accumulated, the eruptive activity spread centrifugally to more and more distant points until at the end of the Pliocene it had reached Middle California.

Thus it seems to me that the four questions suggested by the phenomena of the old river beds and their fillings, have been not only each answered, but they have been all connected together in a satisfactory manner.

Sequence of Events.

It may be well to briefly recapitulate the main points of my view, by narrating rapidly the events in the order of their sequence.

Immediately after the birth of the Sierra Nevada, at the beginning of the Cretaceous period, a drainage system commenced to be formed. This system we may be assured remained unchanged during the whole Cretaceous and Tertiary times, for, as already seen, river channels are remarkable for their permanency. The result of the river-work of all this time was a system of broad trough-shaped channels separated by low divides usually called the Tertiary or old river system. During all this time I suppose the amount of detritus was so related to velocity that there was neither much erosion on the one hand, nor deposit on the other—though on the whole erosion slowly progressed. Then commenced the glacial cold at the end of the Pliocene, a rising of the Sierra region, and the

* Some Ancient Glaciers of the Sierra. This Journal, vol. v, p. 325, 1873, and vol. x, p. 126, 1875.

high Sierra was mantled with snow and ice and glaciers probably occupied the higher portions of the river troughs, and thus large quantities of loose debris were prepared ready for transportation. Then the ground heat of the impending lava flow melted the ice mantle and caused the rushing overloaded torrents which filled up the river channels. This filling doubtless required many years, perhaps centuries, during which there were alternate partial scourings out and re-fillings, yet the whole was, geologically speaking, a rapid process. Then immediately thereafter occurred the fissuring of the high Sierra, and immense discharges of ashes which, washed down by the still melting snows, formed mud streams, which almost completely filled up the river channels, and often apparently overran the low divides. Immediately following the ash-eruptions, lava streams flooded the mountain-slope and completely obliterated the drainage system. Coincidently with the eruptions, and as their cause, there was a considerable elevation of the Sierra range, and increase of the mountain slope.

The previous drainage having been abolished, glaciers and rivers immediately commenced cutting a new system wholly independent of the previous one, though having the same general direction. In cutting these new channels the rivers seem to have shown a preference for the old divides, because there the lava was either wanting or thinner than elsewhere. As a result of the increased elevation of the Sierra, as well as an increase of the causes which produced the Glacial epoch, the reign of ice now reached its culmination. At this time not only was the high Sierra ice mantled and all its cañons filled with glaciers, but even the much lower Coast Range was snow-capped, and glaciers probably ran down its valleys nearly or quite into the Bay of San Francisco.* As another result of the increased elevation of the Sierra and the prodigious consequent erosion by ice and water of this time, and of water alone in subsequent times, the erupted lava was swept clean away from the greater portion of the high Sierra, leaving only the roots visible in the form of dikes, and the river channels lower down the slope were cut far below the detritus-filled and lava-capped old river channels, which are thus left high up on the present divides. Meanwhile meteoric waters percolating downward through the decomposing lava caps, and therefore charged with carbonates of soda and lime, and therefore also dissolving silica, cemented the gravels and petrified the drift wood, or else taking more silica, changed in places volcanic and slate pebbles and bed rock into clay.

Berkeley, Cal., Oct. 15, 1879.

* I have found what I regard as good evidence of glacial action about the site of the University, 300 feet above the Bay of San Francisco.

ART. XXIV.—*Note on the Age of the Green Mountains;* by JAMES D. DANA.

HAVING in the new edition of my Geology, as in the preceding, referred the epoch of the formation of the Green Mountains, that is, of the folding, upturning and crystallization of its rocks, to the close of the Lower Silurian, I here present a fuller statement of my reasons for this conclusion. A further study of the stratigraphy of the eastern part of the region is required to establish its correctness beyond question; but I believe that the following facts and considerations will be found to be strongly in its favor.

By the term Green Mountains, I mean the swell of land with its ridges, about N. 16° E. in trend, which lies between the Connecticut River on one side, and Lake Champlain and Hudson River on the other, and reaching in the south to New York Island. All the rocks of the area thus bounded are not referable genetically to the range; for it is well known that the "Highland" region of Archæan rocks extends over the most of Putnam County, New York, and the southern border of Dutchess County; and that rocks of the same age constitute areas to the east of north of this Highland region, in Connecticut, and also farther north in Massachusetts and Vermont. These Archæan areas introduce difficulties into the geology of Western New England. But the Taconic range and the associated limestones are a known base in the study of the stratigraphy; and by working from it, the difficulties will quite surely be ultimately surmounted. For the sake of the present discussion, the Green Mountain region may be regarded as consisting of (1) a Western section, which includes the Taconic range or belt, and the associated bands of limestone, together with *conformable* formations of slate and schists; (2) a Central mountain section, separately distinguishable only in Vermont; and (3) an Eastern section. The mountain section in Vermont contains the highest summits of the Green Mountains, and is the part to which the name was given. In Massachusetts, the highest peaks are in the more western Taconic range; but still the greatest mean height lies south by west of the mountain belt of Vermont.

In the following pages, the evidence reviewed is arranged under the following heads:

(1) The extent to which the Western belt is a *known* region as regards geological age.

(2) The relations in rocks and stratification between the Western belt and those east of it.

(3) The occurrence of unconformable Upper Silurian rocks within the limits of the region or on its borders.

(4) The magnitude of *an individual* among mountains.

1. *The extent to which the Western belt is a known region as regards geological age.*—The facts on this point are presented in the writer's former papers,* and need not be here repeated. They establish (1) *by means of fossils*, the discoveries of Wing and others in Vermont, and of Dale, Dwight and the writer in Dutchess County, N. Y., and (2) *by the conformability of the strata*, the Lower Silurian age of the Taconic schists and of the associated limestones and schists, eastward to the easternmost of the limestone bands, and westward through Dutchess County, New York, to (and somewhat beyond) the Hudson River. Through their *conformability*, these strata show that all are one in system of dislocation and one in epoch of mountain-making; that the several Lower Silurian formations, from the Hudson River group to the Primordial, are involved in one system of conformable and simultaneously upturned beds.†

The width of this *known* region is, in the southern half of Vermont, 15 to 20 miles, or two-fifths of the width of the State; in Massachusetts, 15 to 20 miles: in Connecticut, 12 to 15 miles; in Dutchess County, N. Y., west of Connecticut, 23 to 25 miles, reckoning to the Hudson River.

The width for Western Connecticut and Dutchess County together is 35 miles, which is about half the whole width between the Hudson River and the Connecticut. North of Dutchess County, in Columbia and Rensselaer Counties, N. Y., the rocks are a continuation of the slates of Dutchess County, and are conformable to the Taconic, according to Mather and Emmons, and hence they belong to the same system. Consequently, the width through these counties and Western Massachusetts is 40 miles, or, again, half the distance from the Hudson to the Connecticut. The rocks are also Lower Silurian in Washington County, up to Whitehall, as represented in the geological maps of Hall and Logan; and in the northern half of Vermont, to its northern boundary.

It thus appears, that, of the mass of land which topographically belongs to the Green Mountain range, that part which is *already proved* to be Lower Silurian in age, and of one geo-

* This Journal, III. v, vi, 1873, xiii (Wing's discoveries), xiv, 1877, xvii, 1879.

† The conformability between the Taconic schists and the adjoining limestone I have observed at various localities in Vermont, Massachusetts, Connecticut and New York, part of the localities on the west side of the range and the rest on the east; and between the same limestone and the schists (including mica schists and gneiss) and quartzyte to the eastward also at several localities in Vermont, Massachusetts and Connecticut, so that the fact cannot rightly be questioned. The discoveries of fossils near Newburgh, *west* of the Hudson, by Whitfield and Dwight, are additional evidence as to the relation of the rocks.

logical and orological system constitutes nearly one-half of the whole range. The part of the Western belt which is not included in the above, is (exclusive of the Archæan area) the southern, or Westchester County, whose connection with the system has not yet been clearly made out.

I. Western Section of the Green Mountain Region.

2. *Rocks; Stratification.*—In mentioning the kinds of rocks we pass from north to south, and from west to east; the former direction follows the strike of the rocks and of the ridges, and the latter, transverse lines.

It is important to note, in reading the following, that hydromica schist and mica schist are essentially the same in composition, the former differing chemically from the latter only in the presence of a few per cent of water (not usually over 5). Physically the difference is wider; the hydromica schist being a fine-grained glossy slate (shading often into a variety that is *called* argillyte from its clay-slate aspect, and which is used as a roofing slate), and feeling more or less unctuous or talc-like.* This roofing slate from Fair Haven, near Castleton, Vermont, one of its most noted localities, is only a more finely grained hydromica schist; for in recent determinations of the alkalies made for the writer by Prof. O. D. Allen of the Sheffield Scientific School of Yale College, and by Mr. O. E. Atwater of the same School, the amount is as great as has been obtained for the hydromica schist, Prof. Allen finding potash 4·61 and soda 1·58 per cent, and Mr. Atwater, potash 4·62, and soda 1·64; and, moreover, the slate fuses rather easily to a light-colored slag. It is therefore a *hydromica argillyte*, or, better, *hydromica phyllyte.*

(1) In Vermont, unaltered and partially altered fossil-bearing Lower Silurian limestones, shales and sandstones occur along the western border of the State.

The Taconic range, next east, commences near Middlebury as a belt of roofing slate, the hydromica phyllyte just mentioned; but it becomes, to the southward, a belt of well characterized hydromica schist, yet with small beds of quartzyte in some places.

Next east of the main band of crystalline limestone (east of the meridian of Rutland), exists a second band of hydromica schist, which, in the southern half of the State, is formed largely of quartzyte; thick strata of quartzyte and hydromica schist alternate, and the former often graduates vertically and in the direction of the bedding into the latter by insensible shadings. Owing

*It is the so-called talcose schist and talcoid schist of the Vermont Report, which is shown in that Report (pp. 503–508, 522) to be non-magnesian. In this paper the terms of the Report are changed in all cases to hydromica schist.

to its greater hardness, high peaks consist of the quartzyte, and these have given the impression that this is the only constituent of the ridges—one not sustained either by the descriptions of Hitchcock in the Vermont Report, or by the writer's observations. The hydromica schist passes into chloritic, magnetitic and feldspathic varieties, and into a hydromica gneiss.*

(2) *South of Vermont.*

In New York, west of Massachusetts, occur slates like argyllite in aspect, but, largely, glossy slates which are hydromica schist; and the latter is the prevailing rock of Dutchess County, except toward its eastern border.

The rocks of the Taconic belt in Massachusetts are, besides ordinary hydromica schist, chloritic and garnetiferous varieties of it, and then in Connecticut, mica schist and staurolitic and garnetiferous schists; and farther south in Eastern New York, toward and below Pawling, 200 miles and more from the commencement of the band in Vermont, micaceous gneiss and gneiss. Again: The Vermont belt of hydromica schist and quartzyte, east of the main belt of limestone, continues through Massachusetts, with the quartzyte still a prominent feature. But the interstratifications, on going southward, are of quartzyte *with mica schist and true gneiss*, instead of with hydromica schist. The mica schists and gneiss are at first fine-grained varieties, but pass into coarser and well characterized kinds in Southern Massachusetts and in Connecticut.

The variation in the rocks on passing southward is very gradual; the extremes are 200 miles or more apart. In going eastward toward the central mountain belt, the same changes are passed through in an interval of 30 to 50 miles.

These different kinds of rocks in this western section are throughout in *conformable* strata, as already stated.

II. Eastern Section of the Green Mountain Region.

The following remarks are confined almost wholly to Vermont. Since the highest of the Green Mountain summits are in this State, we might here look for the strongest contrasts in the rocks on going eastward to the mountain section and beyond it, *unless all the three sections are of one mountain system.*

According to the geological map of Vermont and the descriptions in the Report by C. H. Hitchcock, a band of hy-

* C. H. Hitchcock describes well these gradations in the Vermont Report (pp. 507–509). He says "every grade from soft nacreous schist to sandstone may be found." In Cambridge, he says (p–524), there occur, besides hydromica schist, hydromica varieties of gneiss, sandstones and conglomerates, and plumbaginous and epidotic varieties of hydromica schist; also, all along the western part of Vermont (p. 529), and on the Connecticut River, it passes insensibly into clay slate. The writer observed similar facts in his study of the region.

dromica schist extends through the State on the east side of the mountain section, much like that on the west side; moreover, near the *northern* boundary of the State the two join and are one, showing thereby the closest relation between them. The eastern belt contains to the south some small beds of quartzyte and to the north as well as south, many localities of steatite and serpentine; and the western, while including much quartzyte, has to the north some small beds of steatite. The eastern becomes micaceous to the south and passes into mica schist; Mr. C. H. Hitchcock speaks in the Vermont Report (p. 510) of the gradual change to micaceous rocks, and accounts for it on the general principle that "the rocks grow more metamorphic as we proceed southward from the Canada line."

East of this eastern band of hydromica schist there is a band called clay slate, having parallel relations to that which exists in the western section (Taconic belt). Farther east, there are in the northern two-thirds of the State (besides some local granite areas), a north-and-south belt of mica schist ("Calciferous mica schist"), with some gneiss, and, beyond this, another of argillyte, with a small band of true hydromica schist in some parts near the Connecticut.

III. The Central Mountain Belt.

The mountain belt, which is only one to four miles wide in the northern three-fifths of the State, is marked gneiss on the geological map. But the descriptions in the text, and facts from other sources, show that for the northern fifty miles of its length the principal rock is not "Green Mountain gneiss;" that the most common kinds to the north are *hydromica schist* and *chloritic hydromica schist.*

In the far north, a few miles from the Canada boundary, stands Jay Peak, 4,018 feet in height above the sea level; it consists of hydromica schist much like that of the region adjoining on the east and west.* Thirty miles to the south there is Mt. Mansfield, 4,430 feet high (Guyot), the highest summit in the mountains; and it consists chiefly of mica schist, but in part is made of hydromica schist and chloritic hydromica schist,† and specimens of the latter gathered by the writer from the summit are not distinguishable from those of the true Taconic in Massachusetts. Fifty miles to the south is Camel's Hump, another of the high peaks, 4,088 feet high (Guyot), which consists, according to Adams, and observations by Mr. E. S. Dana, of mica schist; it is stated by Hitchcock

* Vermont Report, p. 523; Adams's Report, 1845. C. H. Hitchcock says (Rep. p. 600) of the Green Mountain gneiss "in Massachusetts, it was called mica schist because of the scarcity of feldspar in it. So here and in many parts of the Green Mountains, it might be appropriately called feldspathic mica schist."

† Vermont Report, 507–509.

to be gneissoid rock, meaning apparently gneissoid mica schist.*

It appears then, that even the mountain belt of Vermont in its northern half consists of rocks much like those of the region either side; and they are essentially the same for the rest of the belt, with this difference, that to the south the mica schist is replaced to a large extent by gneiss.

Again, according to the sections in the Vermont Geological Report and the text describing them, the *schists of the mountain belt are conformable* with the hydromica schist on the east and west.

In Massachusetts, the schists east of the limestone belt are mica schist (as in the Hoosac Mountain) and micaceous gneiss, and they are conformable, according to Hitchcock and my own imperfect observations, with the beds of the limestone belt. But an Archæan area extends northward from Connecticut, and, fifteen miles west of Pittsfield, interrupts the series. C. H. Hitchcock states in the Vermont Report (p. 462) that the mica schist of Hoosac Mountain is a continuation of the Green Mountain belt of gneiss, and that a diminution in the amount of feldspar makes the difference.

In Connecticut the rocks need more study before general conclusions can be positively stated, owing in part to the Archæan areas which give complexity to the subject in the northern half of the State.†

Connection between differences in the rocks and geographical distribution.—As has been shown, marked differences exist between the rocks of the north and south, and between those of the west and east; but the differences are so systematically distributed along the range that they are testimony to its essential unity. The differences are in the main just those that would have come for the most part, from differences in the grade of metamorphism. Intenser metamorphism should be naturally looked for along or about the axis of the range than to the west of it; and so, also (as shown by the facts connected with the Appalachians), on the eastern side of the axis than on the western; and thus it is in reality.

In view of such facts we may safely hold that if to the south, as in Westchester county, the rocks are mainly gneiss, this alone would not be sufficient evidence of difference of system or age.

* A letter from S. R. Hall and Z. Thompson, in Adams's Report (1845), says that there is no true gneiss east of a north and south line from Lake Memphremagog to the western line of Northfield.

† One fact may have much importance when the investigations are completed, namely: that on the west of New Haven, and therefore the west side of the Connecticut Valley depression, the rocks are, *first*, hydromica schist, which is in part almost argillyte in aspect, and in part chloritic, and is undistinguishable from that of the Taconic Mountains and of Mt. Mansfield; next a fine-grained garnetiferous mica schist; next, coarse gneiss and mica schist; and *all are strictly conformable.* The chloritic hydromica schist west of New Haven includes some metamorphic labradorite-dioryte, a rock I have not found in the Taconic region of Berkshire.

IV. Unconformability of Upper Silurian beds.

The three ranges of facts bearing on this point are the following:

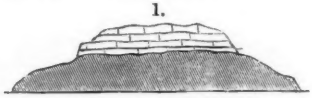
(1) The absence of Upper Silurian strata from among the conformable formations of the Green Mountains.

(2) The existence, near the *western* border of the mountain range, of fossiliferous Lower Helderberg limestone (upper divison of the Upper Silurian) resting *unconformably* on the upturned Hudson River slates.

(3) The existence of fossiliferous Lower Helderberg beds, and in some places Upper Helderberg, near the *eastern* border of the mountain range, in the Connecticut valley, resting *unconformably* on slates or schists which may be of Lower Silurian age, if not of the period of the Hudson River group.

The cases of unconformability between the Lower Helderberg limestone and the Hudson River slates on the *western* side of the mountains occur in the Hudson River Valley, not far from the river, and are described and illustrated in Mather's New York Report (1843). The following figures are from his plates; they represent two cases east of the Hudson River

1.



Becraft's Mt., Columbia Co.

2.

Mount Bob, Columbia Co.

and four just west of it. Fig. 1. Becraft's Mountain, east of the city of Hudson (from Mather, plate 24), in which fossiliferous Lower Helderberg beds are nearly horizontal; fig. 2. Mt. Bob (from plate 38) a few miles south of Becraft's; fig. 3

3.

Between Glasco and the Great Falls of the Esopus, Ulster Co.

4.

Right bank of the Rondout, opposite Wilbur, Ulster Co.

(from plate 7), in Ulster county, west of the river, between Glasco and the Great Falls of Esopus Creek; fig. 4 (from plate 26), in Ulster county, on the right bank of the Rondout; fig.

5.

Two miles northwest of Cocksackie, Greene County.

6.

Two or three miles west of Cocksackie, Greene County.

5 (from plate 8), in Greene county, two miles northwest of Cocksackie; fig. 6 (from plate 8), the same, two or three miles

west of Cocksackie. Mather describes another section on Pine Mountain, between Rondout and Kingston Point, where a high cliff of limestone overlies unconformably the gray grits of the Hudson slate series, the slates dipping 40° to 60° to the east-southeast, and the limestone 80° to the west-northwest. Other sections, from near Rondout, are given in the paper by Mr. T. Nelson Dale, in vol. xviii of this Journal (1879), p. 293.

Special descriptions of the localities are not here necessary. From the observed facts it was inferred by Logan that a very extensive formation of Lower Helderberg limestone once spread over the Hudson River valley; and it is certain that the beds were laid down, as he recognized, after the slates of the Hudson River region and other conformable rocks had been upturned; and since the slates are now proved to be of the Hudson River group, the making of a large part, if not the whole, of the Green Mountains preceded the era of the Lower Helderberg. The occurrence of Niagara limestone—the Coralline or Stromatopora limestone—at the Rondout locality (see the paper referred to by Mr. Dale, and Dr. Barrett's note in the same volume) is evidence, further, that the epoch of upturning or mountain-making preceded the Niagara period, which is the first of the Upper Silurian; and hence that it occupied the interval between the Lower and Upper Silurian.

The faulting and folding of the Upper Silurian strata prove the occurrence of later disturbances, which affected also the underlying unconformable slates; and these, as Mr. Dale suggests, may have taken place at the time of the Appalachian disturbance after the Carboniferous, and have been a consequence of the action which raised the Catskill Mountain plateau.

On the *eastern* side of the mountains the case of unconformable superposition of Upper Silurian fossiliferous rocks occurs at Bernardston, in northern Massachusetts, west of the Connecticut. The facts, first made known by Hitchcock, are given in detail in articles by the writer in this Journal.* The Lower Helderberg age of the fossiliferous beds and their unconformability to the underlying argillyte are admitted by all writers on the subject. These underlying slates are made conformable, in the Vermont Report, with the calcareous mica schist and these, as already stated, with the hydromica schist farther west, where the two are in contact. This point of conformability needs, however, further study before it is received as established. The underlying argillyte, although resembling that west of the mountain belt, is, therefore, not yet *proved* to be Lower Silurian. The reader can review the various considerations bearing on the question, and derive his own conclu-

* Third Series, vi, 339, 1873; and xiv, 379, 1877.

sion as to the probability in the case. The Crinoidal limestone at Bernardston is overlaid by quartzyte and fine grained mica schist which, since they cover a Lower Helderberg limestone stratum,may be metamorphic strata of the Oriskany group, or the lower beds of the Upper Helderberg. The similar beds of quartzyte and mica schist, with conformable beds of amphibolyte, staurolitic mica schist, and quartzytic gneiss and syenyte, representing the same formation, extend up the Connecticut valley. At Littleton, 120 miles to the north, east of the Connecticut River, occur beds of limestone containing fossil corals and Brachiopods of the Upper Helderberg; and on the northern borders of Vermont occur corals of the same age at Owl's Head, on the borders of Lake Memphremagog. These Lower and Upper Helderberg beds were made, as their positions and limits show, in an arm of the sea, which reached from the St. Lawrence region down what is now the Connecticut valley, and they seem to indicate, in connection with other facts, that the valley was defined at an epoch not long preceding the Lower Helderberg, or at that of the making of the Green Mountains.

V. The Magnitude of an Individual among Mountains.

A mountain-individual comprises all the elevations or results of upturnings and flexure that were produced over a continuous region in one mountain-making process; all that was made at one birth, or was monogenetic. Thus the mountain-individual called the Appalachian range comprises in its breadth not merely the various ridges and valleys that make the mountains west of the Blue Ridge, but the Cumberland Table Land and its extension northward to what was once a Catskill Table-land; and in its length it reaches from Central New York to Central Alabama. It has this great extent and yet it is one of the smallest of the earth's mountain individuals. Mountain individuals are necessarily large because they depend on movements in the earth's crust; and, if the crust were no more than twenty-five miles in thickness, deep bendings much less than 100 miles in span and some hundreds in length, would be, from a physical point of view, hardly a possibility. A very large area is therefore required for a mountain individual of folded rocks. It is hence natural, in the case of a mountain-individual along western New England, to look for a breadth at least equal to that of the region which the Green Mountains topographically cover. The Green Mountain region is a northern portion of the Appalachian system and these views make it simply a portion in which the mountain-making occurred at an earlier epoch, having been hastened, as

the writer long since suggested, by the existence on the west, near by, of the stable Archæan area of the Adirondacks.

In conclusion, I repeat the considerations that have been presented:

1. The *western* half of the region between the Connecticut River valley and the Hudson River, that is, the western half of the Green Mountain area, is proved to consist of rocks that are (1) of Lower Silurian age; and (2) of one orological system.

2. The schistose rocks of the eastern half in Vermont are to a large extent similar to those of the western.

3. The rocks of the central mountain section in Vermont are, in its northern part, identical schists (hydromica, etc.), with those on the east and west sides of it.

4. The western border of the region in the Hudson River valley has itsfolded or upturned Hudson River (Lower Silurian) slates, overlaid unconformably by Niagara and Lower Helderberg (Upper Silurian) beds.

5. The eastern border of the region in the Connecticut valley at Bernardston, in Massachusetts, Vernon in Vermont, and the adjoining part of New Hampshire, has Lower Helderberg beds overlying, unconformably, folded or upturned roofing slates (similar to those on the western side), the Lower Silurian age of which is not improbable; and at Littleton in New Hampshire, and on Lake Memphremagog, in northern Vermont, occur unconformable Upper Helderberg (Lower Devonian) beds with fossils.

6. A mountain-individual of folded rocks is necessarily one of great magnitude.

In view of these various considerations, the evidence, although not yet beyond question, is manifestly strong for embracing the whole region between the Connecticut and the Hudson (and to an unascertained distance beyond) within the limits of the Green Mountain synclinorium.

Art. XXV.—*On a New Action of the Magnet on Electric Currents;** by E. H. Hall, Fellow of the Johns Hopkins University.

Sometime during the last University year, while I was reading Maxwell's Electricity and Magnetism in connection with Professor Rowland's lectures, my attention was particularly attracted by the following passage in vol. ii, p. 144:

"It must be carefully remembered, that the mechanical force which urges a conductor carrying a current across the lines of magnetic force, acts, not on the electric current, but on the conductor which carries it. If the conductor be a rotating disk or

* From the American Journal of Mathematics, vol. ii., 1879.

a fluid it will move in obedience to this force, and this motion may or may not be accompanied with a change of position of the electric current which it carries. But if the current itself be free to choose any path through a fixed solid conductor or a network of wires, then, when a constant magnetic force is made to act on the system, the path of the current through the conductors is not permanently altered, but after certain transient phenomena, called induction currents, have subsided, the distribution of the current will be found to be the same as if no magnetic force were in action. The only force which acts on electric currents is electromotive force, which must be distinguished from the mechanical force, which is the subject of this chapter."

This statement seemed to me to be contrary to the most natural supposition in the case considered, taking into account the fact that a wire not bearing a current is in general not affected by a magnet and that a wire bearing a current is affected exactly in proportion to the strength of the current, while the size and, in general, the material of the wire are matters of indifference. Moreover in explaining the phenomena of statical electricity it is customary to say that charged bodies are attracted toward each other or the contrary solely by the attraction or repulsion of the charges for each other.

Soon after reading the above statement in Maxwell I read an article by Prof. Edlund, entitled "Unipolar Induction" (Phil. Mag., Oct., 1878, or Annales de Chemie et de Physique, Jan., 1879), in which the author evidently assumes that a magnet acts upon a current in a fixed conductor just as it acts upon the conductor itself when free to move.

Finding these two authorities at variance, I brought the question to Prof. Rowland. He told me he doubted the truth of Maxwell's statement and had some time before made a hasty experiment for the purpose of detecting, if possible, some action of the magnet on the current itself, though without success. Being very busy with other matters however, he had no immediate intention of carrying the investigation further.

I now began to give the matter more attention and hit upon a method that seemed to promise a solution of the problem. I laid my plan before Prof. Rowland and asked whether he had any objection to my making the experiment. He approved of my method in the main, though suggesting some very important changes in the proposed form and arrangement of the apparatus. The experiment proposed was suggested by the following reflection:

If the current of electricity in a fixed conductor is itself attracted by a magnet, the current should be drawn to one side of the wire, and therefore the resistance experienced should be increased.

To test this theory, a flat spiral of German silver wire was inclosed between two thin disks of hard rubber and the whole placed between the poles of an electromagnet in such a position that the lines of magnetic force would pass through the spiral at right angles to the current of electricity.

The wire of the spiral was about $\frac{1}{2}$ mm. in diameter, and the resistance of the spiral was about two ohms.

The magnet was worked by a battery of twenty Bunsen cells joined four in series and five abreast. The strength of the magnetic field in which the coil was placed was probably fifteen or twenty thousand times H, the horizontal intensity of the earth's magnetism.

Making the spiral one arm of a Wheatstone's bridge and using a low resistance Thomson galvanometer, so delicately adjusted as to betray a change of about one part in a million in the resistance of the spiral, I made from October 7th to October 11th inclusive, thirteen series of observations, each of forty readings. A reading was made with the magnet active in a certain direction, then a reading with the magnet inactive, then one with the magnet active in the direction opposite to the first, then with the magnet inactive, and so on till the series of forty readings was completed.

Some of the series seemed to show a slight increase of resistance due to the action of the magnet, some a slight decrease, the greatest change indicated by any complete series being a decrease of about one part in a hundred and fifty thousand. Nearly all the other series indicated a much smaller change, the average change shown by the thirteen series being a decrease of about one part in five millions.

Apparently, then, the magnet's action caused no change in the resistance of the coil.

But though conclusive, apparently, in respect to any change of resistance, the above experiments are not sufficient to prove that a magnet cannot affect an electric current. If electricity is assumed to be an incompressible fluid, as some suspect it to be, we may conceive that the current of electricity flowing in a wire cannot be forced into one side of the wire or made to flow in any but a symmetrical manner. The magnet may *tend* to deflect the current without being able to do so. It is evident, however, that in this case there would exist a state of stress in the conductor, the electricity pressing, as it were, toward one side of the wire. Reasoning thus, I thought it necessary, in order to make a thorough investigation of the matter, to test for a difference of potential between points on opposite sides of the conductor.

This could be done by repeating the experiment formerly made by Prof. Rowland, and which was the following:

A disk or strip of metal, forming part of an electric circuit,

was placed between the poles of an electro-magnet, the disk cutting across the lines of force. The two poles of a sensitive galvanometer were then placed in connection with different parts of the disk, through which an electric current was passing, until two nearly equipotential points were found. The magnet current was then turned on and the galvanometer was observed, in order to detect any indication of a change in the relative potential of the two poles.

Owing probably to the fact that the metal disk used had considerable thickness, the experiment at that time failed to give any positive result. Prof. Rowland now advised me, in repeating this experiment, to use gold leaf mounted on a plate of glass as my metal strip. I did so, and, experimenting as indicated above, succeeded on the 28th of October in obtaining, as the effect of the magnet's action, a decided deflection of the galvanometer needle.

This deflection was much too large to be attributed to the direct action of the magnet on the galvanometer needle, or to any similar cause. It was, moreover, a permanent deflection, and therefore not to be accounted for by induction.

The effect was reversed when the magnet was reversed. It was not reversed by transferring the poles of the galvanometer from one end of the strip to the other. In short, the phenomena observed were just such as we should expect to see if the electric current were pressed, but not moved, toward one side of the conductor.

In regard to the direction of this pressure or tendency as dependent on the direction of the current in the gold leaf and the direction of the lines of magnetic force, the following statement may be made:

If we regard an electric current as a single stream flowing from the positive to the negative pole, i. e., from the carbon pole of the battery through the circuit to the zinc pole, in this case the phenomena observed indicate that two *currents* parallel, and in the same direction, tend to repel each other.

If, on the other hand, we regard the electric current as a stream flowing from the negative to the positive pole, in this case the phenomena observed indicate that two *currents* parallel and in the same direction tend to attract each other.

It is of course perfectly well known that two *conductors*, bearing currents parallel and in the same direction, are drawn toward each other. Whether this fact, taken in connection with what has been said above, has any bearing upon the question of the absolute direction of the electric current, it is perhaps too early to decide.

In order to make some rough quantitative experiments, a new plate was prepared consisting of a strip of gold leaf about 2 cm. wide and 9 cm. long mounted on plate glass. Good con-

tact was insured by pressing firmly down on each end of the strip of gold leaf a thick piece of brass polished on the underside. To these pieces of brass the wires from a single Bunsen cell were soldered. The portion of the gold leaf strip not covered by the pieces of brass was about 5½ cm. in length and had a resistance of about 2 ohms. The poles of a high resistance Thomson galvanometer were placed in connection with points opposite each other on the edges of the strip of gold leaf midway between the pieces of brass. The glass plate bearing the gold leaf was fastened, as the first one had been, by a soft cement to the flat end of one pole of the magnet, the other pole of the magnet being brought to within about 6mm. of the strip of gold leaf.

The apparatus being arranged as above described, on the 12th of November a series of observations was made for the purpose of determining the variations of the observed effect with known variations of the magnetic force and the strength of current through the gold leaf.

The experiments were hastily and roughly made, but are sufficiently accurate, it is thought, to determine the law of variation above mentioned, as well as the order of magnitude of the current through the Thomson galvanometer compared with the current through the gold leaf and the intensity of the magnetic field.

The results obtained are as follows:

Current through gold leaf strip. C.	Strength of Magnetic field. M.	Current through Thomson galvanometer. c.	$\frac{C \times M}{c}$
·0616	11420 H	·00000000232	303000000000·
·0249	11240 "	·00000000085	329000000000·
·0389	11060 "	·00000000135	319000000000·
·0598	7670 "	·00000000147	312000000000·
·0595	5700 "	·00000000104	326000000000·

H is the horizontal intensity of the earth's magnetism = ·19 approximately.

Though the greatest difference in the last column above amounts to about 8 per cent of the mean quotient, yet it seems safe to conclude that with a given form and arrangement of apparatus the action on the Thomson galvanometer is proportional to the product of the magnetic force by the current through the gold leaf. This is not the same as saying that the effect on the Thomson galvanometer is under all circumstances proportional to the current which is passing between the poles of the magnet. If a strip of copper of the same length and breadth as the gold leaf but ¼ mm. in thickness is substituted for the latter, the galvanometer fails to detect any current arising from the action of the magnet, except an induction

current at the moment of making or breaking the magnet circuit.

It has been stated above that in the experiments thus far tried the current apparently tends to move, without actually moving, toward the side of the conductor. I have in mind a form of apparatus which will, I think, allow the current to follow this tendency and move across the lines of magnetic force. If this experiment succeeds, one or two others immediately suggest themselves.

To make a more complete and accurate study of the phenomenon described in the preceding pages, availing myself of the advice and assistance of Prof. Rowland, will probably occupy me for some months to come.

Baltimore, Nov. 19th, 1879.

It is perhaps allowable to speak of the action of the magnet as setting up in the gold leaf strip a new electromotive force at right angles to the primary electromotive force.

This new electromotive force cannot, under ordinary conditions, manifest itself, the circuit in which it might work being incomplete. When the circuit is completed by means of the Thomson galvanometer, a current flows.

The actual current through this galvanometer depends of course upon the resistance of the galvanometer and its connections, as well as upon the distance between the two points of the gold leaf at which the ends of the wires from the galvanometer are applied. We cannot therefore take the ratio of C and c above as the ratio of the primary and transverse electromotive forces just mentioned.

If we represent by E′ the difference of potential of two points a centimeter apart on the transverse diameter of the strip of gold leaf, and by E the difference of potential of two points a centimeter apart on the longitudinal diameter of the same, a rough and hasty calculation for the experiments already made shows the ratio $\frac{E}{E'}$ to have varied from about 3,000 to about 6,500.

The transverse electromotive force E′ seems to be, under ordinary circumstances, proportional to Mv, where M is the intensity of the magnetic field and v is the velocity of the electricity in the gold leaf. Writing for v the equivalent expression $\frac{C}{s}$ where C is the primary current through a strip of the gold leaf 1 cm. wide, and s is the area of section of the same, we have $E' \propto \frac{MC}{s}$.

November 22d, 1879.

ART. XXVI.—*Measures of the Polar and Equatorial Diameters of Mars, made at Princeton, New Jersey, U. S.;* by Professor C. A. YOUNG.

THE polar compression of Mars has never yet been satisfactorily determined, the results of different observers ranging from that of Sir W. Herschel, who made it $\frac{1}{16}$, to that of Bessel, who found it insensible. The value $\frac{1}{89}$, deduced by Main at Oxford, from his measures in 1862–3, has probably been of late more generally accepted than any other, though by no means without reserve.

Hartwig, in his recently published investigation upon the diameters of Venus and Mars, gives $\frac{1}{96}$ as the result obtained by combining all the double-image measurements made at Königsberg, Leiden, Oxford, Berlin, Paris and Strassburg.

Either of these values, however, is apparently irreconcilable with the planet's known mass and period of rotation, if we admit the presence of water upon its surface, as the polar "snow caps" seem to indicate, except upon the almost absurd assumption of a density rapidly increasing from the center toward the surface.

It has seemed to the writer quite possible that the difference of illumination of the limbs of the planet, caused by phase, may lie at the bottom of the difficulty. Except on rare occasions there is phase enough, even at the moment of opposition, to produce a notable difference of appearance between the fully illuminated edge of the planet's disc and that opposite, a difference which can hardly fail to be felt in micrometric measurements. Unexceptionable observations for determining the polar compression can therefore be made only when the planet reaches opposition and its node together. This was so nearly the case last season, that on the night of November 12th, an observer on the planet would have witnessed a Transit of the Earth. At this time, and for a few days before and after, the phase was extremely small, and an opportunity was presented for determining the planet's ellipticity such as will not be available again for nearly half a century.

The measures detailed below were made by myself with a filar position micrometer attached to the nine and one-half inch Equatorial of the School of Science Observatory. The object glass of this instrument (by Clark) is constructed substantially upon the Gaussian curves, and is of the highest excellence. During the past year it has shown repeatedly both of the satellites of Mars, the two outer satellites of Uranus, and the Saturnian satellite Mimas, the last, for my eye, which is not extremely sensitive, being just at the limit of visibility. The teles-

cope doubles distinctly, and on fine nights easily, the little companion of α^2 Capriconi, nor has it yet failed upon any of the objects usually considered tests for twelve-inch instruments.

While perfectly aware that measures by a filar micrometer are subject to a considerable constant error, the measure of an illuminated disc being always excessive, I have thought they might safely be used in determining a difference of diameter in different directions. The wires were dark upon an illuminated field. The magnifying power throughout the observations was 398. The value of one revolution of the micrometer screw at 50° F., as determined by about a hundred star transits, is $17''{\cdot}860 \pm 0''{\cdot}003$. The screw appears to be a very perfect one without sensible errors of periodicity or run.

Marth's ephemeris, in the Monthly Notices for June, 1879, was used in setting the position circle and in computing the minute phase-corrections. Each measure of a double diameter consisted of twenty settings of the micrometer wire, five with the movable wire on one side of the fixed wire, ten with it upon the other side, and finally five more in the first position. The correction for thickness of the wires was determined by a full set of readings at each of the three different fixed wires, once at least in each evening's work.

After each measurement of either diameter, the wires were turned 90° by the position circle, and the other diameter was measured. In some cases, however, after finishing a set of four measures (two of each diameter) at one of the fixed wires, the movable wire was transferred to another of the fixed wires, and the same diameter was then measured again as the first of a new set. The measures thus come in pairs, each measure of the polar diameter being matched with an equatorial immediately preceding or following. The only exceptions to the above statements are the asterisked measures of November 12th, which consisted of but *ten* settings each, and the polar diameter No. 23 (marked with a dagger) which has no strictly corresponding equatorial measure, work having been interrupted by clouds. A supplementary equatorial measure, No. 58, was therefore taken to equalize the numbers.

All the measures which were made are given, without omission or alteration of discordant readings. The weights assigned are on a scale of ten for a perfectly unexceptionable observation, and were determined from the atmospheric and other conditions at the time of observation. The total number of micrometer readings, exclusive of those for determining the thickness of wires, was 1140.

Had not cloudy weather prevented, the series would have been much more extensive. The nights of November 11th and 13th were entirely overcast; on November 12th only a

Equatorial Diameter of Mars, November, 1879.

No.	Date	Hour Angle	Angle with Vertic.	Measured Diam.	p	$\rho+\phi$ +	ω ±	I −	D_ω	pvv
	Nov.	h m	°	″					″	
2	7	−3 58	+74·5	20·824	9	+35	−200	−045	20·614	·0004
4	7	−2 00	+84·6	·814	9	+34	−200	16	·632	0
6	10	−3 45	+75·0	·702	6	10	− 83	44	·585	14
8	10	−3 03	+77·1	·887	8	10	− 84	37	·776	162
10	10	−0 47	−74·3	·779	8	10	− 83	46	·660	6
12	10	+0 02	−51·2	·878	5	12	− 84	106	·700	22
13	10	+0 54	−27·7	·954	6	14	− 84	149	·735	61
15	10	+1 31	−15·9	·722	6	16	− 83	162	·493	119
*17	12	−0 11	−57·9	·812	3	2	0	89	·725	25
19	14	−1 13	−83·4	·361	6	0	+133	19	·475	152
21	14	−0 25	−64·5	·415	7	1	+133	72	·477	165
24	15	−3 14	+76·7	·740	7	10	+219	39	·930	613
26	15	−2 32	+80·4	·683	5	9	+219	28	·883	310
29	15	−0 26	−64·9	·301	8	10	+215	71	·455	256
31	15	+0 03	−50·6	·289	8	12	+215	107	·409	225
32	15	+1 20	−18·5	·535	9	16	+217	159	·609	6
34	15	+1 55	−10·6	·960	4	17	+223	165	1·035	641
36	16	−3 52	+75·2	·063	3	17	+304	43	·341	257
38	16	−3 15	+76·8	·324	8	17	+309	38	·612	4
41	16	−0 54	−77·7	·660	8	16	+315	36	·955	824
43	16	−0 22	−62·9	·869	4	17	+318	77	1·127	972
44	16	+0 15	−44·8	·285	7	20	+309	119	·495	135
46	16	+0 41	−33·0	·084	6	21	+306	141	·270	795
49	16	+1 53	−11·1	·555	8	24	+313	165	·727	69
51	16	+2 18	− 6·8	·426	9	25	+310	167	·594	14
52	16	+2 29	− 5·3	·615	7	25	+313	167	·786	162
55	17	−3 38	+75·7	·051	4	25	+396	41	·431	165
56	17	−3 23	+76·3	·159	7	25	+398	40	·542	60
†58	17	−2 52	+78·6	20·066	6	24	+396	− 33	·453	197

Equatorial Diameter, $20''\cdot634 \pm 0\cdot035$. $a = \text{Io} = 0''\cdot168 \pm 0''\cdot050$.

single pair of measures of small weight could be obtained, and on the 14th only two pairs and an odd one. On all the other nights there were also occasional periods of atmospheric disturbance or entire interruptions. It is especially to be regretted that more measures were not obtained at considerable western hour angles, as they would have gone far to eliminate the effects of atmospheric dispersion and astigmatism of the observer's eye. The observations have been very carefully reduced under my direction, by my assistant, Mr. M. McNeil, who deserves and has my warmest thanks for his interest and help in the matter.

In the accompanying tables the first column contains the reference number of the observation; the second gives the date, the third the hour angle of the planet, and the fourth the angle between the measured diameter and the vertical. The fifth column contains the measured diameter, corrected for thickness of wires and reduced to arc, and the sixth, headed p, gives the assigned weight. The seventh column, $(\rho+\varphi)$, contains the sum of the corrections for refraction and phase in

Polar Diameter of Mars, November, 1879.

No.	Date	Hour Angle	Angle with Vertic.	Measured Diam.	p	$\rho+\phi$ +	ω ±	I −	D_ω	pvv
	Nov.	h m	°	″					″	
1	7	−4 08	−16·0	20·801	9	+24	−200	−161	20·464	0·0070
3	7	−2 20	− 8·8	·684	9	15	−199	−166	·334	427
5	10	−4 08	−15·4	1·066	4	19	− 83	162	·840	332
7	10	−3 24	−14·0	·906	6	14	− 84	165	·671	88
9	10	−1 12	+ 6·6	·793	8	8	− 84	167	·550	0
11	10	−0 23	+26·3	·810	8	6	− 82	151	·583	8
14	10	+1 11	+68·3	·805	8	2	− 83	62	·662	97
16	10	+1 53	+78·9	·585	7	1	− 82	32	·472	45
*18	12	0 00	+37·7	1·041	3	0	0	133	·908	380
20	14	−0 56	+12·4	·359	7	7	+133	164	·335	330
22	14	−0 10	+32·6	·351	7	5	+133	142	·347	294
†23	14	+1 33	+74·5	·459	7	1	+133	45	·548	0
25	15	−2 51	−11·0	·932	4	11	+222	166	·999	799
27	15	−2 14	− 7·2	·819	4	10	+220	167	·882	436
28	15	−0 42	+18·0	·247	7	7	+214	160	·308	416
30	15	−0 10	+32·8	·272	7	7	+215	141	·353	277
33	15	+1 39	+75·9	·680	7	2	+219	41	·860	664
35	15	+2 11	+82·1	·286	5	1	+215	23	·479	26
37	16	−3 32	−14·1	·497	5	17	+312	163	·663	62
39	16	−2 57	−12·0	·452	9	15	+312	164	·615	36
40	16	−1 06	+ 9·0	·620	5	11	+313	166	·778	255
42	16	−0 41	+18·6	·702	4	11	+314	159	·868	400
45	16	+0 29	+51·8	·417	9	7	+310	104	·630	55
47	16	+0 54	+62·2	·336	9	6	+309	78	·573	4
48	16	+1 40	+76·2	·058	6	5	+306	40	·329	298
50	16	+2 06	+81·3	·181	7	4	+307	25	·467	50
53	16	+2 47	+86·6	·172	7	4	+307	10	·473	43
54	17	−3 54	−14·9	·268	8	22	+400	162	·528	5
57	17	−3 12	−13 0	20·100	7	18	+398	−164	·352	280

Polar Diameter, $20''\cdot552 \pm 0''\cdot043$. $C = 0''\cdot082 \pm 0\cdot035$. Polar compression $= \frac{1}{21\cdot9}$.

units of the third decimal; the eighth column (ω) gives the reduction to opposition, or the correction necessary to reduce the observed diameter to what it would have been if the planet were at the same distance from the earth as at the instant of opposition. The next column, headed I, contains a correction, which will be explained below, depending upon the inclination of the measured diameter to the vertical; the column $D\omega$ contains the concluded opposition-diameter, and the last column, the product obtained by multiplying the square of each residual by the weight. For the polar diameter $\Sigma pv^2 = 0\cdot6177$, for the equatorial, $0\cdot6435$: both together $= 1\cdot2612$.

If we apply the corrections for refraction, phase, and reduction to opposition, and group the results according to the angle made by the observed diameter with the vertical, it at once becomes evident that a further correction is needed, horizontal lines being systematically measured smaller than the same when vertical. Thus, if we take the twenty measures of the equatorial diameter made when its inclination to the vertical was more

than 45°, we get for the mean (by weights) $20''{\cdot}687 \pm 0''{\cdot}028$ at a mean angle of 72°·4. The nine nearly vertical measures, give $20''{\cdot}780 \pm 0''{\cdot}044$ at a mean angle of 19°·3. Similarly the ten nearly horizontal measures of the Polar diameter give $20''{\cdot}608 \pm 0''{\cdot}031$ at a mean angle of 73°·8, while the nineteen nearly vertical measures give $20''{\cdot}706 \pm 0{\cdot}926$, at a mean angle of 16°·9. No sensible difference appears between results obtained when the measured diameter inclines to the right, and those obtained when the inclination is toward the left.

It is not certain what causes produce this difference between horizontal and vertical measures, but it is probably due in part to atmospheric dispersion, and partly to a known vertical astigmatism of the observer's eye. In either case the effect would be proportional to the cosine of the inclination, and we may therefore assume with sufficient approximation, that the correction required to an observation is $I = \alpha \times \cos\beta$; β being the angle with the vertical, and α a constant to be determined from the observations themselves.

Approximate values of α can be obtained from the data given above; thus from the two groups of equatorial measures, (regarding each group as a single measure) we get $\alpha = 0''{\cdot}149$. The two polar groups give $\alpha = 0''{\cdot}146$; a coincidence which strongly attests the reality of the correction.

It is not, however, strictly correct to treat these groups as single observations. The more legitimate course is to regard each observation, (corrected for refraction and phase and reduced to opposition) as furnishing an equation of condition of the form

$$\textit{measured diameter} = \textit{true diameter} + \alpha \cos \beta.$$

This has been done, and from the equations of condition, using the weights given, normals have been formed, in several different ways.

Thus we may take as the unknown quantities π, ε, (the polar and equatorial diameters) and α. Or calling $c = \varepsilon - \pi$, we may take as the unknowns α, π and c, or α, ε and c. Or finally, putting $m = \frac{\varepsilon + \pi}{2}$, we may form our normals with m, c and α.

Whichever of the four sets of normals we use, we get for the unknown quantities involved, the following values, weights and probable errors, viz:

ε (Equatorial Diam., Nov. 12, 1879)	$= 20''{\cdot}634 \pm 0''{\cdot}034$	wt. 8·92
π (Polar Diam., " " ")	$= 20{\cdot}552 \pm 0{\cdot}043$	wt. 5·85
$m = \frac{\varepsilon + \pi}{2}$	$= 20{\cdot}593 \pm 0{\cdot}035$	wt. 8·85
$c, = (\varepsilon - \pi)$	$= 0{\cdot}0818 \pm 0{\cdot}0348$	wt. 8·75
α coefficient of inclination correction	$= 0{\cdot}168 \pm 0{\cdot}050$	wt. 4·17

The introduction of the correction I reduces the sum of pv^2 from 1·3994 to 1·2612.

The values of the diameters are of course not very reliable, being subject to the considerable constant error which always affects filar micrometer measures. If we accept Hartwig's determination, 9″·352,* for the diameter at distance unity, a value which depends upon the whole body of heliometer and double-image micrometer measures up to 1877, we get for the opposition diameter of 1879, 19″·128. Comparing this with my mean, we find 1″·46 as the constant error of my filar micrometer measures, a value rather unexpectedly large, but not unprecedented.

This constant error can however have but very slight, if any, influence upon the measured difference of diameters, and we find for the ellipticity of the visible periphery of the planet, the value $\frac{0''\cdot0818}{19\cdot128}=\frac{1}{234}$.

As the pole was not on the periphery, but according to Marth's ephemeris at a distance of 14°·5 from it, a slight correction is still needed to deduce the true polar compression. The diameter measured as polar was that joining two antipodal points in Areographic latitude 75°·5. Recalling the formula for the radius of a spheroid at any latitude, viz:

$$\rho = a\sqrt{\frac{1-2\,\varepsilon^2\sin^2\varphi+\varepsilon^4\sin^2\varphi}{1-\varepsilon^2\sin^2\varphi}},$$

and putting $\frac{\rho}{a}=\frac{233}{234}$ we easily find ϵ, the eccentricity of a planetary meridian, and from it C_0, the polar compression. Performing the solution we get $C_0=\frac{1}{231}$, which may be taken as the final result of the work here detailed.

The limits of probable error extend from $\frac{1}{168}$ to $\frac{1}{364}$, and so far as can be judged from the observations themselves, the chances are more than two to one that the ellipticity lies between $\frac{1}{139}$ and $\frac{1}{515}$.

The work of reduction was nearly finished before the writer had seen Professor Adams's recent paper upon the orbits of the satellites of Mars, in which he gives $\frac{1}{325}$ as the ellipticity which the planet ought to have if it follows the same law of central density as the earth. The closeness of the accordance is probably in considerable degree accidental, but it is quite satisfactory.

Princeton, New Jersey, Jan., 1880.

* The mean diameter 20″·593 resulting from my observations, when reduced to distance unity, gives 10″·068; a value sensibly identical with that obtained by Peirce from a discussion of the Washington Mural circle observations, and used in the American Ephemeris.

ART. XXVII.—*On the Use of the Sine-formula for the diurnal Variation of Temperature;* by B. A. GOULD.

IN the "Jahresbericht des physikalischen Centralobservatoriums für 1877 und 1878," Dr. H. Wild, the eminent director of that institution, has recently published some deservedly severe remarks upon the large amount of time and labor which is unprofitably spent in making observations with inaccurate instruments, and calculations with inaccurate or insufficient data. Probably there is no physicist who has occupied himself with meteorological studies to any extent without finding his attention forcibly attracted to the same considerations; and it would be needless to add any testimony in support of the general tendency of Dr. Wild's remarks.

But to my surprise I find a foot-note appended to them, which I transcribe in the original, lest I do it injustice in the attempt to translate:

"Ich kannte damals noch nicht den eben erst erschienenen Band mit den physikalischen Beobachtungen der amerikanischen Expedition der "Polaris," wo Herr E. Bessels das Abschreckendste in ganz unnützen Berechnungen aller meteorologischen Elemente nach der Bessel'schen Formel geleistet hat. Schade um die Zeit, die so für eine bessere Verwerthung der schönen Beobachtungen verloren ging. Die Palme der Leistungen in dieser Richtung gebührt freilich Hrn. Gould, der es in dem erst erschienenen, im übrigen sehr werthvollen ersten Bande der 'Anales de la Oficina Meteorólogica Argentina' zu Stande gebracht hat, für Buenos Aires, aus bloss 3 Beobachtungen am Tage, 4 Constante der Bessel'schen Formel zu bestimmen, und damit den täglichen Gang der Temperatur, des Barometerstandes, der Bewölkung, etc. zu berechnen."

Had this criticism emanated from any one whose scientific position and services in meteorology were less distinguished, or whose opinions seemed entitled to less deference than is deservedly conceded to those of Dr. Wild, I should not reply to it. But, under the circumstances, I ask leave to call attention to one or two considerations which he has overlooked in the sarcastic intimation that I had undertaken to determine four unknown quantities from three equations.

As answer, I might, it is true, cite the facts; since the series of observations made for many years at 8 A. M., 2 P. M. and 8 P. M.,—a part of which is printed on pages 314 to 359 of the volume referred to,—were incorporated in the computations as well as the series made at 7 A. M., 2 P. M. and 9 P. M. One hour being common to both systems, it was easy to determine the small constant correction needed for making the two sys-

tems congruous. And it will be seen that in determining the diurnal variation of the temperature (pp. 412–414) and its expression by the goniometric formula, five daily observations were really employed instead of three only as stated by Dr. Wild.

It is, however, not this circumstance which leads me to make a public rejoinder; but the general principle involved. For as regards the diurnal *barometric* variation, the constants of the sine-formula have in truth been deduced from the results of only three daily observations. By reference to page 427, it will be seen that I intentionally disregarded the results of the two others on account of their apparent want of congruity with the other series made by a different observer and with a different instrument. Here Dr. Wild's implied censure might find application from his point of view; and it is with reference to this that I desire to reply.

There are, in these cases, conditions attendant upon the problem which serve the purpose of additional equations. Even were the so-called formula of Bessel to be regarded solely as a means of interpolation, as Dr. Wild seems to suppose it, the conditions that the daily maximum of temperature occurs within two or three hours after noon, and the minimum not very long before sunrise, furnish the requisites for deducing four constants from observations made at proper intervals three times a day. In determining the diurnal variation of the barometer, the attention of the reader is, on page 427, directed to this circumstance, and the two hypotheses are distinctly stated upon which the values of the constants depend. It appears to me that any just criticism of the investigation must rather be directed against the propriety of these hypotheses, than against the legitimateness of the method.

There is yet another consideration which, although not fully set forth in the printed volume, has guided me in the course which I have pursued, and in which I purpose to continue, in the investigation of the climatic constants for other points in South America. It is that observations made at other hours and not very remote places accord in confirming the general form of the diurnal curves, which is the only hypothetical assumption. The observations made before the "Oficina Meteorológicæ" was established, but especially the series which have for some years been carried on at this observatory, afford such corroboration. The monthly means of the hourly observations made here during the last two years are very well represented by the sine-formula with only three variable terms. The object of the investigations is to attain a knowledge of the true laws. If there really are laws, they must be capable of expression by some algebraic formula; and numerous tests

have shown that the sine-formula affords such expression more conveniently than any other known to me; so that the well-established general form of the diurnal curves in these regions affords the only requisite in addition to the three daily observations for a trustworthy determination of the constants. The only possible objections to this procedure are: either that the diurnal curve is in fact not a true curve, or that the assumptions relative to its form are too uncertain to take the place of an equation of condition. That, in these regions at least, both of these objections would be untenable it would be extremely easy to prove.

In his treatise upon the Temperatures of the Russian Empire, published in the Supplementary volume of the "Repertorium für Meteorologie," Dr. Wild has taken the positions—1st, that the sine-formula represents no law whatever in the diurnal variation, but is solely a means of interpolation; and 2d, that the mean daily changes of temperature cannot be expressed by any simple curve. And he gives practical expression to these opinions by totally abandoning the employment of all general formulas, even for those places where the observations are made hourly. It is apparently in advocacy of this singular doctrine that he has made the scarcely courteous comments which prompt the present remarks.

Even were the formula in question solely one of interpolation, I cannot see how in this respect the eye and free hand of a draughtsman could be more correct. Yet it would seem that Dr. Wild intends to assert this, and that he believes that the moments of mean daily maximum and minimum can be better determined by graphical approximation than by numerical determination.

Far be it from me to object to graphical methods. There is no doubt of the fact that in various investigations for which much more nicety is requisite than is attainable in determining atmospheric temperatures, such methods are most serviceable. But, so far as my knowledge extends, it has never before been asserted that they afford greater accuracy than numerical ones. And, putting this question aside, how can the excessive minuteness with which the temperatures at given moments of local time at the various stations have been referred to moments of Göttingen time, by reductions calculated to hundredths of a degree, be supposed to afford a corresponding accuracy, unless there be reason to suppose that the observations were in general really made at the instants recorded,—that the clock even was always sharply determined, the observers unfailingly punctual, and at least the observed tenths of degrees reasonably trustworthy. (See "Supplement Band," p. 36.) In such cases we cannot even suppose a compensation in the errors of obser-

Differences (0.– C.) between the mean hourly temperatures observed at Cordoba and those corresponding to Bessel's formula for the daily variation.

	1877. Dec.	1878. Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	1879. Jan.	Feb.	Mar.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Mean.
0h	+0°04	−0°15	−0°03	−0°15	+0°13	+0°02	+0°16	+0°21	+0°06	0°00	+0°17	−0°14	−0°15	+0°01	+0°02	+0°18	+0°10	+0°17	+0°23	+0°31	−0°01	+0°11	−0°03	+0°06
1AM	−0·16	+0·14	+0·27	+0·18	+0·53	+0·09	+0·25	+0·08	+0·14	−0·11	−0·30	+0·04	−0·20	−0·31	−0·13	+0·07	−0·03	+0·21	+0·17	+0·29	−0·01	+0·03	+0·13	+0·06
2	−0·05	−0·16	+0·04	+0·17	−0·05	−0·14	−0·26	−0·09	−0·24	−0·35	−0·34	+0·14	−0·04	−0·09	−0·19	−0·20	−0·29	−0·06	−0·03	−0·18	−0·04	−0·23	+0·55	−0·09
3	+0·34	+0·03	−0·08	−0·21	−0·36	−0·15	−0·10	−0·22	−0·10	−0·05	+0·35	+0·07	+0·35	+0·09	+0·06	−0·17	−0·25	−0·16	−0·13	−0·39	+0·12	+0·01	+0·68	−0·01
4	+0·14	+0·01	−0·02	−0·07	0·00	+0·16	+0·20	+0·23	+0·08	+0·12	+0·37	+0·20	+0·12	+0·32	+0·23	+0·32	−0·06	−0·13	−0·02	+0·23	−0·25	+0·22	+0·23	+0·11
5	−0·12	0·00	+0·28	+0·22	+0·26	+0·05	−0·01	+0·01	+0·13	+0·45	+0·07	−0·12	−0·35	−0·01	+0·08	+0·14	+0·11	+0·23	−0·05	+0·14	+0·17	+0·04	−0·62	+0·05
6	+0·17	+0·14	0·00	−0·06	+0·45	+0·45	−0·01	+0·21	−0·11	+0·10	−0·80	−0·36	0·00	−0·50	−0·33	−0·21	+0·04	+0·14	+0·34	+0·20	+0·11	−0·16	−0·95	−0·05
7	+0·27	−0·07	−0·20	−0·09	−0·53	−0·54	−0·07	−0·05	−0·28	−0·40	−0·03	+0·13	−0·12	−0·01	−0·35	−0·48	−0·21	−0·35	−0·26	−0·24	−0·22	−0·34	−0·44	−0·21
8	+0·22	−0·20	+0·01	+0·12	−0·05	−0·25	−0·13	−0·27	+0·13	+0·56	+0·61	+0·37	+0·52	+0·38	+0·39	+0·41	+0·09	−0·04	−0·16	−0·29	+0·01	+0·50	+0·40	+0·14
9	+0·14	+0·22	+0·08	−0·03	+0·25	+0·08	+0·32	+0·21	+0·17	+0·34	+0·29	+0·30	−0·26	+0·24	+0·31	+0·42	−0·09	+0·19	+0·29	+0·31	+0·22	−0·08	+0·49	+0·13
10	−0·11	−0·02	+0·02	+0·05	+0·03	+0·26	−0·13	+0·31	−0·06	−0·35	−0·47	−0·59	−0·12	−0·37	−0·28	−0·41	−0·33	+0·04	0·00	+0·20	−0·22	+0·03	+0·36	−0·03
11	+0·11	+0·15	−0·32	−0·12	−0·14	−0·03	−0·23	−0·04	−0·02	−0·34	−0·30	−0·04	−0·03	−0·38	−0·43	−0·25	−0·24	−0·13	−0·27	−0·38	−0·03	−0·40	+0·38	−0·15
12M.	+0·07	−0·25	+0·02	+0·07	−0·36	−0·01	+0·41	−0·46	−0·19	+0·09	+0·12	+0·17	+0·08	+0·51	+0·26	+0·21	+0·26	+0·01	+0·10	+0·17	+0·27	+0·19	−0·06	+0·07
1PM	+0·11	−0·02	−0·18	−0·07	−0·18	+0·18	−0·18	+0·39	+0·15	−0·27	+0·22	−0·07	+0·05	+0·07	+0·24	+0·20	0·00	+0·05	+0·30	+0·01	−0·33	+0·14	−0·38	+0·02
2	−0·09	+0·21	+0·20	+0·15	+0·12	0·00	+0·12	+0·16	+0·34	+0·24	+0·28	+0·25	−0·04	−0·02	+0·07	−0·07	+0·14	+0·04	−0·28	0·00	+0·31	+0·03	−0·49	+0·07
3	−0·12	+0·02	−0·32	−0·04	−0·05	−0·08	−0·29	−0·42	−0·33	−0·39	−0·43	−0·05	+0·20	−0·24	−0·24	−0·04	+0·19	−0·10	−0·07	−0·21	−0·19	−0·18	−0·34	−0·16
4	−0·35	−0·18	+0·02	−0·09	+0·07	−0·25	+0·30	+0·15	+0·07	+0·05	−0·02	−0·34	−0·25	+0·13	−0·08	−0·23	+0·16	+0·12	+0·15	+0·12	+0·05	−0·07	−0·05	−0·02
5	+0·31	+0·13	+0·19	+0·20	+0·04	−0·02	−0·01	−0·07	+0·23	+0·29	+0·12	+0·02	−0·07	−0·02	+0·09	+0·19	+0·07	+0·03	0·00	+0·09	+0·11	+0·12	+0·37	+0·10
6	−0·45	−0·27	−0·48	−0·38	−0·43	−0·13	−0·20	−0·10	−0·12	+0·06	+0·13	+0·18	+0·09	−0·07	+0·18	+0·09	−0·26	−0·09	+0·13	−0·04	−0·17	+0·12	+0·71	−0·07
7	−0·09	+0·34	+0·30	+0·18	+0·29	+0·08	−0·11	+0·10	+0·03	+0·06	−0·14	−0·08	+0·13	+0·36	+0·18	−0·13	+0·09	−0·10	−0·29	−0·17	+0·03	−0·13	+0·45	+0·06
8	0·00	0·00	+0·18	+0·14	+0·03	+0·31	+0·29	−0·25	−0·02	+0·32	+0·14	−0·16	+0·02	−0·27	−0·24	−0·06	+0·16	+0·23	−0·05	−0·14	+0·10	+0·03	−0·19	+0·03
9	−0·28	−0·20	−0·30	−0·11	+0·08	+0·30	−0·05	+0·19	+0·10	−0·21	−0·28	+0·09	−0·25	−0·27	−0·18	+0·09	+0·16	+0·01	+0·39	+0·56	−0·02	+0·03	−0·44	−0·03
10	−0·10	−0·01	+0·31	+0·19	−0·11	−0·20	−0·07	−0·12	−0·18	+0·03	+0·11	+0·16	+0·10	+0·19	+0·23	+0·03	+0·18	−0·01	+0·01	−0·23	−0·04	−0·07	−0·42	−0·06
11	0·00	+0·14	+0·01	−0·25	−0·02	−0·18	−0·20	−0·16	+0·02	−0·23	+0·13	−0·17	+0·22	+0·23	+0·11	−0·10	+0·01	−0·30	−0·50	−0·36	+0·03	+0·06	−0·34	−0·06
$\sqrt{\Sigma v^2}$	0·19	0·16	0·20	0·16	0·25	0·22	0·20	0·22	0·16	0·27	0·31	0·22	0·19	0·26	0·23	0·22	0·18	0·15	0·22	0·25	0·17	0·18	0·43	0·09
Ampl.	10·8	10·6	10·5	7·9	9·7	12·3	9·8	14·2	12·4	13·4	13·7	12·5	13·6	14·8	11·7	9·5	10·7	12·3	13·2	11·1	12·3	14·7	10·5	

vation, inasmuch as the effect of a given error in the time differs at different seasons. Yet, in spite of all this, these insignificant minutiæ seem to be thought worthy of application to data obtained by the free hand of a draughtsman, rounding off errors in the observations, while the representation and generalization of the results by a formula accompanied by an exact exhibit of its discordances from the several observations, is deemed inadmissible.

If the epochs of daily minimum, as deduced from monthly means of hourly observations by the two methods, differ (as at Katharinenburg, for example) by amounts varying between +57 and −71 *minutes*, I cannot agree with the distinguished author in attributing a large share of such discordances to errors occasioned by the use of Bessel's formula; nor even in preferring the results of the graphical method. It seems much more probable that the phenomenon ought to be attributed to a very different class of influences. It is at least certain that what the draughtsman obtains without gauge or measure in equating out a series of data, the formulas will afford together with accurate indications of the probable errors. Yet if in the numerical computations the manifest uncertainties and contradictions of inaccurate data may not be equated out, while on the other hand, the draughtsman's line may be boldly drawn across and through all such irregularities, it is highly probable that this latter method may furnish a line which represents the true law with a nearer approximation. When not too many terms are employed, the general formula gives correctly what the graphical process only gives approximately; and by a glance at the residuals the computer sees what additional degree of accordance with the data may be attained by the introduction of an additional term. Finally, upon the hazardous assumption that all the data are faultless, so that only an accurate interpolation is needed, any number of observations may be represented with absolute precision by the formula. Yet recourse to a computation in which twelve variable terms are used to represent twenty-four hourly observations is very nearly equivalent to a declaration that the computer believes no periodic law to exist.

That entirely similar discordances present themselves between the times of maxima and minima, as obtained by the two methods, for other stations than the one cited can cause no surprise. The only surprising fact in the case is that the inference should have been drawn that the employment of a cyclical formula is inadmissible even when it represents the entire series of observations within the limits of reasonable error.

Still retaining the hypothesis already mentioned regarding the character of the service rendered by the sine-formula, a

great advantage must certainly be derivable from a general expression which absolutely represents such observations as exist, even though it might not be demonstrably correct for intermediate moments. Objections may be urged against the abuse or improper application of such a formula, but there is certainly a legitimate use for it. An empirical periodic function can be made to represent any number of observations of any periodic fluctuation, provided the number of constants be equal to that of the independent data. Whether it does or does not, at the same time represent the true law throughout its extent is a separate question. But surely the attainment of a general expression which represents the available observations is most important, since the higher terms serve to make manifest the degree of confidence to which the lower ones are entitled.

If, however, with an inferior number of constants, a satisfactory representation can be obtained, we have an argument to a corresponding extent, that the periodic variation does in fact follow the law expressed by the formula. This ceases to be simply an implement of interpolation, whenever the number of its constant is less than is logically required by the data and the conditions which it adequately satisfies. Consequently if the coëfficient of the third term is small in a formula which well represents a given number of observations, we have thus a measure of the trustworthiness of the preceding terms, and at the same time are informed what epicyclical term suffices to satisfy the existing series. But when a large number of successive observations, in all parts of the cycle can be sufficiently well represented by a small number of terms, this fact gives evidence that the true law is expressed by the formula. This is the only basis for a legitimate application of Bessel's formula to the diurnal variation of temperature. If it represents the true law, it ought to be used, since it affords the most general and convenient expression.

The monthly means of the hourly observations of temperature at Cordoba have been found capable of representation with great accuracy by sine-formulas with only three variable terms. The average mean discordance for a single hour, since November, 1877, is 0·28° or scarcely greater than the error to be apprehended in a single observation, when all sources of error are considered. For the barometer this discordance is less than 0·05°. We have thus the evidence that the formula represents the true law of nature with close approximation; and the degree of approximation is furthermore attested by the character of the residuals, irrespective of their magnitude. If the mental constitution of the investigator is such that he prefers to have a definite numerical expression for the degree of approximation, he may find this in the subsequent terms. I consider it

fully justifiable to employ a similar formula for discussing a small number of daily observations made at places not too remote and in which the topographical conditions are not too different. The results so obtained thus far fully confirm this view, and especially those relative to the variations of temperature.

In his treatise on the temperature of the Russian Empire, Dr. Wild arrives at a conclusion which he expresses as follows (p. 95): "For interpolating omitted hours, especially those of the night, the formula of Lambert and Bessel can,—according to Theorem 11 regarding the form of the curve of the diurnal period in the temperature,—only be applied, at most, for entirely maritime climates. For all places of which the situation is in any degree continental, it must be totally rejected, on account of the sudden bend in the curve at the time of sunrise, which it is only capable of representing by the employment of very many terms (10 or more), even for complete series of hourly observations."

That this conclusion does not hold good for Cordoba will, I think, be manifest to any one upon inspection of the preceding table, which exhibits the residuals between the monthly means of hourly observations and the corresponding values afforded by the monthly sine-formulas with four variable terms. Cordoba is situated about 400 miles from the La Plata in a straight line, and about 620 from the Atlantic, its position being pre-ëminently continental. Buenos Aires, however, for which point my use of the sine-formula is so sharply censured by Dr. Wild, is a seaport.

The residuals which result from the use of only three variable terms are by no means important, though of course somewhat larger than those given in the table. There is a certain tendency to grouping of their signs by triplets which shows that the fourth term may be added with advantage; yet neither their order of magnitude nor the fluctuation of their signs gives token, even in this case, of any such discordance between the formula and the true law of variation as Dr. Wild thinks he has discovered for the Russian stations. Here at least it may be said of the daily curve, "Natura non facit saltum." As regards the magnitude of the residuals, no one can be more conversant than Dr. Wild with the numberless influences which, in spite of every care, affect the accuracy of observations, nor yet with those analogous ones in the varying and intricate combinations of transient atmospheric conditions, which by their perturbations throw a veil over the true fundamental law. And I think he cannot fail to acknowledge that the true law is here expressed by the sine-formula closely

enough to justify its employment for places in this region where the number of daily observations is very much smaller. In order to show the relative as well as the absolute amount of residuals, I have appended the mean amplitude of the observed daily variation for each month.

Finally I am quite ready to concede that, although the mean daily variation in Cordoba follows with much regularity the law expressed by a few terms dependent upon sines of angles proportional to the time of day, it is by no means improbable that in higher latitudes and different circumstances this may not be the case; inasmuch as the simplicity of the law and its corresponding formula may there be essentially affected by perturbations which are not manifest here. Yet even in such case it would be difficult to justify, either from a scientific or from any other point of view, such remarks as those cited at the beginning of this article.

If the efforts of meteorologists to raise their study to the rank of an exact science are to attain that success for which the progress during recent years justifies a hope, this can only be accomplished through the aid of algebraic generalization. The frequent misuse of a method affords no argument against its legitimate employment.

I could wish that these few remarks might tend to convince the eminent Russian meteorologist, that in his zeal against the misuse of the so-called Bessel formula, he is in danger of taking a backward step and obstructing the progress of meteorology by opposing his great influence to the custom of algebraic generalization, which is a necessary condition for the advance of the science from the descriptive to the exact stage. Yet even though this hope should prove unfounded, I cannot but think that he will perceive the very great injustice which he has done to my work. Although there is, under the especial circumstances of the case, a certain grim humor in the imputation of a needless waste of time and labor in deducing illusory results, I shall not allow myself to be drawn into any personal considerations.

But in any case, I must express the earnest hope that the comparatively modern usage of generalizing meteorological results to the utmost by means of algebraic formulas may be stimulated and encouraged in every way.

Cordoba, Argentine Republic, November 9, 1879.

ART. XXVIII.—*On the Chemical Composition of the Uraninite from Branchville, Conn.;* by W. J. COMSTOCK. (Contributions from the Laboratory of the Sheffield Scientific School. No. LVIII.)

THE composition of uraninite, or pitchblende, has never been satisfactorily established, although the formula $U_3O_8 = UO_3 + 2UO_2$, suggested by Rammelsberg, has been generally accepted. The presence of the other elements, shown in the different analyses, has been explained by assuming that the material analyzed was impure. Professors Brush and Dana obtained during their recent explorations at Branchville,* small isometric crystals having a specific gravity of 9·22–9·28, which reacted with the fluxes for uranium, and yielded upon reduction before the blowpipe a globule of lead. They identified the mineral with uraninite and suggested the probability that the lead entered into its composition. The material for analysis was handed to me by them.

The crystals occurred in a small vein in albite; there was nothing with which they could be confounded, and hence the purest material was available for analysis. A few crystals had a thin yellow coating, probably of uranium phosphate. The crystals were all octahedral in habit, in most the planes of the dodecahedron appeared, and in a few cases, those of the cube. The mineral is readily soluble in nitric acid, yielding a yellow solution, but it is not acted upon by hydrochloric acid. It decrepitates on heating and gives off traces of moisture. When heated strongly in an open tube it has a very slight acid reaction on litmus paper. An analysis showed the mineral to have the following composition:

	I.	II.	Mean.
U =	81·67	81·33	81·50
Pb =	4·01	3·94	3·97
Fe =	·41	·39	·40
O =	13·37	13·47	13·47*
H_2O =	·88		·88
Total			100·22

* For the oxygen, the determination in No. II is used, as it was made with greater care.

The uranium, lead and iron were separated and weighed according to the ordinary methods. The water was driven off by ignition and determined by absorption in a calcium chloride tube. The oxygen was determined by decomposing the min-

* This Journal, III, xvi, 35, July, 1878.

eral with sulphuric acid in a sealed tube and titrating with a solution of potassium permanganate, thus affording with the uranium, lead and iron determined, all necessary data for calculation. That this method can be employed with accuracy in presence of both UO_2 and UO_3 was first proved by experimenting upon pure U_3O_8. The best results were obtained by boiling the sulphuric acid used to expel the air, and displacing the air in the tube by CO_2 before sealing. The following are the results obtained.

	Taken.		Found.
1.	·5160 U_3O_8 containing	·1655 UO_2	·1647 UO_2
2.	·5016 U_3O_8 "	·1608 UO_2	·1599 UO_2

Assuming the state of oxidation in which the lead and iron exist in the mineral, the percentages of UO_2 and UO_3 can be calculated from the data furnished by the permanganate titration. The most probable assumption is that they exist as PbO and FeO, replacing the UO_2 by equivalent amounts of $(PbO)_2$ and $(FeO)_2$. The analysis then becomes:

			Ratios.	
UO_2	= 40·08		·1392	·1392
UO_3	= 54·51		·2004	
PbO	= 4·27	$(PbO)_2$	·0095	·2133
FeO	= ·49	$(FeO)_2$	·0034	
H_2O	= ·88			
Total	100·23			

We shall then obtain the ratio—

$$\overset{IV}{R}O_2 : \overset{VI}{R}O_3 = ·2133 : ·1392 = 1·53 : 1 = 3·06 : 2$$

and the composition of the mineral will be represented by the formula—

$$3\overset{IV}{R}O_2 + 2\overset{VI}{R}O_3$$

in which the $\overset{IV}{R}$ represents tetrad uranium replaceable by two atoms of lead or iron, and $\overset{VI}{R}$, hexad uranium.

It is also possible that the iron exists as Fe_3O_4, which, like pitchblende, crystallizes in the isometric system. But under that supposition, the ratio of 3 : 2 would not be essentially changed.

When the mineral is heated in air, it is to be expected that the uranium would be oxidized to the state of U_3O_8, and the iron to Fe_2O_3. Calculated from the analysis, the gain in weight by this oxidation would amount to 1·48 per cent its weight. In determining the water, the mineral was found to weigh ·57 per cent more after ignition; this, added to the water expelled, gives 1·45 per cent.

The mineral may then be considered as a basic uranous uranate, in which the basic uranium is replaceable by lead (and iron). That the uranium exists in pitchblende as U_2O_3, has been assumed without proof; the only related crystallized mineral heretofore analyzed is the questionable uranoniobite of Scheerer, who gives 15·6 p. c. = PbO,Nb_2O_5 and SiO_2, and 2·7 = loss.

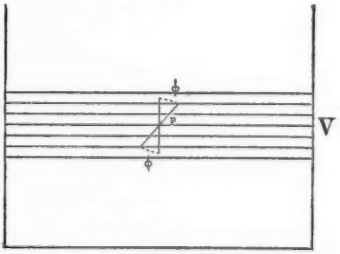
The acid reaction shown in the tube is unexplained. It was first attributed to sulphur, but by careful examination none could be detected.

ART. XXIX.—*On the Mean Free Path of a Molecule;* by N. D. C. HODGES.

THE free path of a molecule is dependent on the amount of obstruction it meets with, on the density of the medium. Meyer gives for the mean free path on page 308 of his Kinetische Theorie der Gase, $L = \frac{1}{\pi\sqrt{2}N\xi^2}$. Here N is the number of molecules in the unit volume.

I consider the length of path in a medium of variable density. At the surface of a liquid, if there is no sharp transition from the liquid to the gaseous state, we shall have a succession of less and less dense vapors from where there is liquid to the surrounding atmosphere. The layers V (fig. 1) are what I refer to. The depth of these vapors, is of course, much magnified.

1.



I propose to find the pressure upon the particle, *p*, when the surface of the liquid is plain and when it is spherical. Taking molecules moving with any definite velocity, they will reach *p* and give it an impulse, when they are at a distance from *p* less than their mean free path. Now, the particles from below come from denser layers than those from above. A greater number will come from below than from above; there will be a tendency to drive *p* upward. To find this tendency, we must find

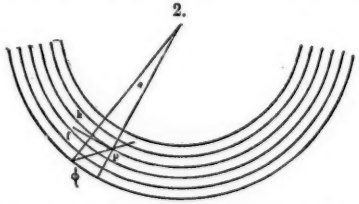
how much denser the lower layers, from which molecules impinging on p come, are than those above, from which particles come to p. If $\delta\rho$ is the obstruction met by a molecule in passing vertically upward through a single layer, $\frac{\delta\rho}{\cos\varphi}$ will be that met in the direction at an angle φ with the vertical. The integral $\frac{1}{\cos\varphi}\int_{\rho_0}^{\rho_1} d\rho$ gives the obstruction met by the molecule from one end to the other of its path. This integral must be constant, for the length of path is independent of the direction. As the differential of the obstruction is the same as the differential of the density,

$$\frac{1}{\cos\varphi}\int_{\rho_0}^{\rho_1} d\rho = \frac{\rho_1 - \rho_0}{\cos\varphi}$$

when ρ_0 is the density at the point p and ρ_1, that at the other end of the path. As $\frac{\rho_1-\rho_0}{\cos\varphi} = \kappa$, the numerator $\rho_1-\rho_0$ must be proportional to $\cos\varphi$. The pressure on p is proportional to this difference, and the resultant component in the upward direction to $\cos^2\varphi$.

When the surface is spherical (fig. 2), each element of the path offers an obstruction expressed by $\frac{\delta\rho}{\cos(\varphi - \frac{1}{2}\alpha)}$ for the parts below p, and by $\frac{\cos(\varphi+\frac{1}{2}\alpha)}{\delta\rho}$ for those above p, α is the angle between the radius of curvature at the point p and that to the

2.



other end of the path. $\varphi-\frac{1}{2}\alpha$ is the mean value of the angle between the direction of the path and the normals to the surfaces of equal density for the parts below p, and $\varphi+\frac{1}{2}\alpha$ the corresponding angle for those above p.

Integrating, $\frac{\rho_1'-\rho_0'}{\cos(\varphi-\frac{1}{2}\alpha)} = \kappa = \frac{\rho_0'-\rho_2'}{\cos(\varphi+\frac{1}{2}\alpha)}$. This shows that, whereas the difference in density above and below was the same for a plane surface, in the case of curved surfaces the pressure from below is greater, and that upward less. Or the pressure from below is greater, and that from above greater. This must cause a greater density at p.

The tendency for a particle to move from the liquid into the surrounding atmosphere is due to the difference in density of its liquid and of its vapor. For small changes in the density, the change in this tendency may be assumed as proportional to the change of density. It must be found what change in density takes place at p. As the change in density is due to an increase of pressure on p, this increase must be equal in all directions. So it is only necessary to consider one direction. Take the direction tangent to the curved surface at p. The increase in pressure is, therefore, proportional to the difference in density of the layer through f and that through h, or to the length fh. It is evident that $\frac{fh}{\mathrm{L}} = \frac{\frac{1}{2}\mathrm{L}}{r}$, when r is the radius of curvature, and L the length of the mean free path.

Hence we have $\frac{\text{the change of tension}}{\text{the tension of the vapor at plane surface}} = \frac{\frac{1}{2}\mathrm{L}}{r}$

Sir William Thomson has shown that the change in tension at a curved surface is equal to the pressure of a column of the vapor of the height to which the liquid would rise in a capillary tube of a diameter of twice the radius of curvature of the surface.

In a tube of diameter $1{\cdot}294^{mm}$ water rises to a height of $23{\cdot}379^{mm}$. The data for the calculation are:

Weight of a liter of water vapor (at 100° C.)	·80357 grams
" " mercury	13·579 "
Tension of water vapor at 20° C.	$18{\cdot}495^{mm}$

The height of a column of mercury equivalent to the column of water vapor of height $23{\cdot}379^{mm}$ is

$$\frac{{\cdot}80357}{13{,}579}, 100, \frac{18{\cdot}495}{760}, \frac{23{\cdot}379}{100}$$

The first factor is the fraction of a liter of mercury which a liter of water equals. The second reduces the height of this to millimeters. The third gives the result at 20° C., supposing the vapors to follow Boyle's law. The fourth is the fraction of a liter there was to be considered.

The expression for the mean free path in these surface vapors is then

$$\tfrac{1}{2}\mathrm{L} = {\cdot}647\frac{{\cdot}80357}{13{,}579}, 100, \frac{18{\cdot}495}{760}, \frac{1}{18{\cdot}495}\frac{23{\cdot}379}{100}\, r = {\cdot}647.$$

This gives $\mathrm{L} = {\cdot}0000024^{mm}$.

If the law according to which the density of the vapors vary with the depths was known, the free path of a molecule in a gas at the ordinary pressure could be found.

Physical Laboratory, Harvard College, Cambridge, U. S. A., January 27, 1880.

ART. XXX.—*On the Western Limits of the Taconic System;* by S. W. FORD.

SINCE the publication of my paper entitled "Notes on the Primordial Rocks in the vicinity of Troy, N. Y. (this Journal for July, 1871), Professor Dana has repeatedly urged that the Troy Primordial beds, together with those of Bald Mountain, Washington County, and their equivalents farther northward in Vermont and Canada, should not, in strictness, be referred to the Taconic, inasmuch as they were added by Dr. Emmons to his Taconic System several years subsequent to its original definition, and upon insufficient grounds. In this view I have always acquiesced, partly because of its essential justness, and partly because of the endless disputes and controversies which its adoption seemed to me calculated to prevent. In my paper referred to, the Troy beds are spoken of by me as part of the Taconic System, not, however, as a believer in the system, but because they appeared to me to have a better right to this title than to any other that had previously been applied to them. The Taconic System, as a system distinct from the Silurian, has appeared to me from my earliest knowledge of it, of questionable standing; but the chances in favor of its general acceptance seemed to me quite as good so long as it rested within the limits originally assigned to it. In extending his system westward, in 1846, from Petersburg, N. Y., to the Hudson River, Dr. Emmons certainly had the remarkable uniformity of dip and conformability of the rocks over the region studied in his favor; but when he found himself under the necessity of assuming an inversion of the whole system in order to get the black slates of Bald Mountain (in which Trilobites had then recently been discovered) at its summit, grave doubts were justly raised respecting the correctness of his interpretations.

I have no doubt that, to many, this assumption in itself (the typical region considered by Emmons being not far from 45 miles wide), has all along been regarded as practically fatal to the Taconic cause. We now know, thanks to the earnest and enlightened researches of Wing, Dana, Billings, Dale, and others, that the true Taconic rocks represent the Champlain Division at least from the Calciferous to the Hudson River group inclusive, and possibly the division entire; but with regard to the North-and-South belt added to them in 1846 (or that lying between Petersburg and the Hudson), while a considerable part of it has been shown to be Primordial, the age of by far its greater portion is still in doubt. How far the Primordial beds which come up at Troy and Bald Mountain are continued eastward as surface rocks, it is impossible at present to say. To

my mind, one of Dr. Emmons's greatest services to the cause of science was his recognition of the great stratigraphical break running east of the Hudson River by which the Primordial rocks were made to stand above those at the top of the Lower Silurian, and his advocacy of it in spite of the adverse paleontological determinations of Professor Hall, who referred the whole at first to the Hudson River group, and subsequently, at least in part, to the Quebec. Dr. Emmons's pronounced antagonism to the Hudson River doctrine grew, in this case at any rate, out of his appreciation of fundamental differences; and for this signally good work, notwithstanding the failure of his favorite system, he should ever receive, it seems to me, the grateful recognition of all workers in the department.

New York, February 5, 1880.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Vapor-density of Chlorine.*—Victor Meyer has replied to the criticism of Seelheim* upon his experiments on the vapor-density of chlorine at high temperatures. The platinous chloride which was used was heated in a boat. After the operation, this boat contained a rod of solid coherent platinum sponge, having the form of the vessel. Removed with care, its weight was almost precisely that calculated from the $PtCl_2$. No trace of sublimed or crystallized platinum could be detected. As to the experiments of Troost and Hautefeuille, it is difficult to see how platinum heated to a yellow heat in chlorine, is perceptibly volatile, due to the formation of platinic chloride, since platinic chloride is completely dissociated at 600° C. As in all similar reactions, time is of course an important factor; but the author found that a weighed quantity of platinum heated for an hour to about 1570°, in an active current of dry chlorine, lost scarcely one per cent. Hence it can scarcely be supposed that the quantity of metal volatilized during the few seconds required for a vapor density determination, with no gas current, would be at all appreciable.—*Ber. Berl. Chem. Ges.*, xii, 2199, Dec., 1879.

G. F. B.

2. *On the Action of Phosgene gas on Ammonia.*—Fenton has examined the white amorphous substance which is produced when phosgene gas ($COCl_2$) acts upon ammonia, with a view to ascertain whether the urea contained in it is the ordinary form of this body or an isomer of it. For the preparation, the two thoroughly dried gases were brought together in large flasks, the phosgene being prepared by passing carbonous oxide through

* This Journal, III, xix, 65, Jan., 1880.

boiling antimonic chloride. A portion of the white solid was examined by Bouchardat's method, and crystals of guanidine sulphate were prepared from it. Another portion was evaporated to dryness with excess of barium hydrate in vacuo over sulphuric acid, the residue was extracted with absolute alcohol, the solution evaporated to dryness on a water bath, dissolved in water, treated with CO_2, evaporated and digested with absolute alcohol. On evaporation of the alcohol crystals were obtained having all the appearance of urea. Treated with sodium hypobromite and hypochlorite, the nitrogen evolved was nearly twice as much with the hypobromite. Evaporated with silver nitrate, silver cyanate crystallized out. Heated in a tube, ammonia was evolved and the residue gave a strong reaction for biuret. Heated to 40° with urea ferment, its solution became alkaline and gave off ammonia. Nitric acid and mercuric nitrate gave their characteristic reactions. Hence the substance obtained is identical with ordinary urea. Direct tests showed that this urea existed as such in the original white powder, and hence rendered it probable that its isomer, the symmetrical carbamide $CO\left\{\begin{matrix}NH_2\\NH_2\end{matrix}\right.$ is not formed in the reaction.—*J. Chem. Soc.*, xxxv, 793, Dec. 1879. G. F. B.

3. *On the Hydrocarbon Fluoranthene.*—Fittig has published, in conjunction with Liepmann, a second paper upon fluoranthene, a hydrocarbon discovered by him in 1875 and described in 1878. It occurs among the more solid products of the final distillation of coal tar, and has the formula $C_{15}H_{10}$, intermediate between phenanthrene $C_{14}H_{10}$ and pyrene $C_{16}H_{10}$. Separation from pyrene by the fractional crystallization of the picrates being tedious, fractional distillation in a partial vacuum was resorted to with success. The vapor density of the pure fluoranthene was 6·638, theory requiring 6·574 for $C_{15}H_{10}$. On oxidation with chromic acid, diphenyleneketone-carbonic acid is the principal product, mixed, however, with a quinone. By conducting the operation with care, a mixture is obtained on the filter, of the acid, the quinone, and the unattacked hydrocarbon. Removing the acid with sodium carbonate and dissolving the residue in hot alcohol, long, flat, ruby-red, brilliant needles of a compound of the quinone with fluoranthene $C_{15}H_8O_2+(C_{15}H_{10})_2$ separate on cooling. Treatment with hydro-sodium sulphite dissolves the quinone, leaving the hydrocarbon. The quinone crystallizes from alcohol in small red needles, fusing at 188°. Diphenyleneketone-carbonic acid resists oxidation energetically, and hence may be prepared from the crude product from the press. Treated with fuming nitric acid at a gentle heat it gives mononitro-diphenyleneketone-carbonic acid. Fused with potassium hydrate, it gives isodiphenic acid, $C_{12}H_8\left\{\begin{matrix}COOH\\COOH\end{matrix}\right.$ or $\left\{\begin{matrix}C_6H_4COOH\\C_6H_4COOH\end{matrix}\right.$, which on oxidation gives isophthalic acid, $C_6H_4\left\{\begin{matrix}COOH\\COOH\end{matrix}\right.$. Suspended in water, and treated with sodium amalgam, diphenyleneketone-carbonic

acid yields a carboxyl derivative of diphenylenemethane (fluorene), which the authors call fluorenic acid, $\begin{matrix} C_6H_4 > CH_2 \\ | \\ C_6H_3 - COOH \end{matrix}$. This acid, distilled with lime gives the hydrocarbon fluorene $\begin{matrix} C_6H_4 > CH_2 \\ | \\ C_6H_4 \end{matrix}$. Oxidation by chromic acid destroys fluorenic acid, but with permanganate in an alkaline solution, diphenylene-ketone-carbonic acid is formed. From these data the authors assign to fluoranthene the formula $\begin{matrix} C_6H_4 > CH - CH \\ | \qquad\quad \| \\ C_6H_3 \!\!-\!\!-\!\!-\!\!-\!\!- CH \end{matrix}$, and to diphenyleneketone-carbonic acid $\begin{matrix} C_6H_4 > CO \\ | \\ C_6H_3 - COOH \end{matrix}$, isodiphenic acid being as above given.—*Liebig's Ann.*, cc, 1, Dec. 1879. G. F. B.

4. *On the Introduction of Hydroxyl by Direct Oxidation.*—R. Meyer and A. Baur have examined the possibility of converting cumene-sulphonic acid into an oxyacid by direct oxidation while its isomer, propyl-benzene-sulphonic acid resists this action. Normal propyl-benzene was converted into the sulpho-acid, and this, first into the barium and then into the potassium salt. The latter, submitted to permanganate, in solution of potassium hydrate, gave only carbonic acid, the greater part of the salt remaining unchanged. Cumene from cuminic acid was converted into cumene-sulphonic acid, and the potassium salt was submitted to oxidation as in the previous case. A product resulted which was markedly more soluble in alcohol than the salt used, and which afforded on analysis the formula $C_6H_4 \begin{cases} C_3H_7O \\ SO_3K \end{cases}$. Consequently the oxidation had resulted in the substitution of an atom of hydroxyl for one of the hydrogen atoms in the lateral isopropyl chain, producing oxypropylbenzene-sulphonic acid $C_6H_4 \begin{cases} C_3H_6OH \\ SO_3H \end{cases}$. These results are a new confirmation of the view that only hydrogen atoms which occupy tertiary positions can be changed into hydroxyl by a direct oxidation, the isopropyl group containing one such hydrogen atom, the propyl group none.—*Ber. Berl. Chem. Ges.*, xii, 2238, Dec. 1879. G. F. B.

5. *On the Constitution of Dibromethylene.*—There are two bodies of the formula $C_2H_2Br_2$; one acetylene dibromide, and the other dibromethylene. One of these bodies must be symmetrical and have the constitution $\begin{matrix} CHBr \\ \| \\ CHBr \end{matrix}$; the other must be unsymmetrical, and be $\begin{matrix} CH_2 \\ \| \\ CBr_2 \end{matrix}$. Anschütz has demonstrated that it is the acetylene dibromide which is symmetrical, and hence, by inference, the dibromacetylene must be unsymmetrical. To establish this question experimentally, Demole has made use of the fertile

discovery of Friedel and Crafts, that aluminum chloride remarkably facilitates the substitution of a hydrocarbon radical for a haloid element, and mixed together 28 grams dibromethylene with 150 grams benzene, adding gradually to the mixture from 40 to 50 grams of Al_2Cl_6. Hydrobromic acid was disengaged, the reaction proceeding quietly. On fractioning the hydrocarbons obtained two products were collected, one boiling from 270° to 290°, and the other above 350°. The former was an oil, colorless, highly refractive, of an agreeable odor, having the formula $C_{14}H_{12}$, or that of stilbene or diphenyl-ethylene. Its boiling point, and its state as a liquid show it to be the dissymetrical stilbene of Hepp. To prove this dissymetry still further, it was oxidized with chromic acid, saturated with sodium carbonate, and the crystallized sodium salt distilled. The substance obtained boiled at 295°–300°, crystallized on cooling in large white rhombic prisms, fusing about 48°, and was therefore pure benzophenone, $CO\left\{\begin{matrix}C_6H_5\\C_6H_5\end{matrix}\right.$.—*Bull. Soc. Ch.*, II, xxxii, 547, Dec. 1879. G. F. B.

6. *On the Archil-lichens of California.*—Hesse has submitted the archil-producing lichens obtained from California to a chemical examination. He gives the Neue Freie Presse of Vienna credit for the statement in 1871 that 300 persons had gone from New York to Lower California to collect these lichens then lately discovered. They were found on hard, rocky soils near the coast, and a single person could collect a ton, valued at 300 dollars, in four days. In the year 1870 the archil sold in the United States from this source was valued at $14,900, and the extract prepared from it at $4,700. The lichens found their way to the London market, and specimens reached him late in 1871. They had been pronounced to be the well-known lichen *Rocella tinctoria;* but doubting the correctness of this view, Hesse sent specimens to Laurer, the lichenologist, who answered by letter that it appeared to be new, and proposed for it the name *Rocella frutectosa*, placing it between *R. tinctoria* and *R. fuciformis* and nearer the latter. Hesse himself placed it as a variety of *R. fuciformis.* The lichens were extracted with milk of lime, the lime was thrown down by carbon dioxide gas, and the chromogen was purified by crystallization from hot alcohol. When pure it exhibited all the properties of β-erythrin, and gave on analysis the same formula, $C_{20}H_{22}O_{20}$. It is optically inactive and yields picroerythrin and orsellinic ether when decomposed by alcohol. Beside erythrin, the lichen contains very small quantities of rocellic acid. The author regards this as confirmatory of his view as to the species.—*Liebig's Ann.*, cxcix, 338, Nov. 1879. G. F. B.

7. *On the Determination of Sulphur in Coal.*—Nakamura, a student in the Engineering College in Tokio, has devised a new process of determining sulphur in a coal, which seems to work well. It consists in heating the coal below a red heat in contact with alkaline carbonates, by which the coal, whether bituminous or not, rapidly undergoes, without evolution of smoke, complete

atmospheric oxidation in a manner hardly to have been expected. To one part of coal in very fine powder take three or four parts of the mixed alkali-carbonates. Mix intimately in a platinum dish and heat at first gently, using alcohol in place of gas to avoid the sulphur of the latter. Raise the heat very slowly, not reaching a visible red until the surface of the mass becomes faintly gray. Then heat to a faint red, and keep it there for an hour, when the mass will be nearly or quite white. It is then treated with water, filtered, and the sulphuric acid determined in the filtrate as usual. The complete combustion of coal and coke at so low a temperature is noticeable. The carbonate seems to exert no chemical action in the case, but acts mechanically, apparently the spaces between the particles allowing a draft of air, and the combustion proceeding from the bottom toward the top. Direct experiment shows no loss of sulphur, and comparative tests prove the results to be accurate. The complete roasting process occupies about an hour and a half.—*J. Chem. Soc.*, xxxv, 785, Dec. 1879. G. F. B.

8. *A Theoretical and Practical Treatise on the Manufacture of Sulphuric Acid and Alkali, with the Collateral Branches;* by George Lunge, Ph.D., F.C.S., Professor of Technical Chemistry at the Federal Polytechnic School, Zurich (formerly manager of the Tyne Alkali Works, South Shields), London, 1879. Vol. I, pp. 658, 8vo. (John Van Voorst).—Dr. Lunge, in his volume on Sulphuric Acid, explains in full detail, not only the practical steps of the manufacture, but discusses, as only a good chemist can, the theoretical questions involved in the various processes. The work is illustrated with plans drawn to scale, of every part of all the apparatus required, and such clear directions as to construction, and such minute instructions as to operating are given that the book is a complete guide to the manufacturer.

Since the first decade of this century, when sulphurous acid was first conveyed in a continuous stream into the lead chamber, and the steam jet was introduced, and the operation thus made unintermittent, the main features of this, the most important of all the chemical industries have remained unchanged. But the ingenuity of manufacturers has found ample scope in discovering other sources of sulphur than crude brimstone, and devising suitable furnaces for burning them in, and in contriving means for reducing to the utmost the loss of sulphur and nitrogen. Iron pyrites, and, to a limited extent, other sulphuretted minerals and other metallurgical products, have displaced brimstone in most European works. This change was begun in 1838, when the Sicilian Government, by unwisely raising the price of brimstone, drove acid makers to seek a substitute; and it has been consummated by the introduction, since 1860, into every European market of the copper-bearing pyrites of Spain, whose profitable yield of copper allows of a very low price being charged for its sulphur contents. Of this important mineral not less than 600,000 tons are imported annually into Great Britain alone, prima-

rily for the manufacture of acid; but 20,000 tons of metallic copper are extracted from it by leaching—a not inconsiderable lye product. Dr. Lunge expresses surprise that similar mineral, which he supposes to abound in many localities on this side the Atlantic, has not here also taken the place of brimstone. The reason is that no mineral, as suitable, does exist within available distance of our manufacturing centers. It is a question whether Spanish and Portugese pyrites might not be profitably burnt by the Atlantic coast works of the United States. Toward a decision of this question, Dr. Lunge offers ample data by candidly discussing the advantages and disadvantages of brimstone and pyrites respectively as a source of sulphur, and by describing the modifications in plant which the change involves.

Another topic to which Dr. Lunge devotes several chapters, and which merits the consideration of our manufacturers—as there is some misapprehension on the subject—is the best method of adding the nitre and the most efficient mode of preventing its loss. Everywhere the Gay Lussac tower is used to absorb by means of sulphuric acid the nitrous acid escaping from the last chamber; but our manufacturers do not seem yet to have recognized the full value of the Glover tower, when placed between the burner and the first chamber. There is an opinion prevalent that if the sulphurous acid from the burner be used in the tower to decompose the nitrosulphuric acid, the temperature is raised to a degree sufficient to liberate nitrogen and far above that at which the gases should enter the chamber. Dr. Lunge, who was one of the first manufacturers to adopt the invention which Mr. Glover freely offered to the public, shows conclusively that these objections were not valid, and describes how the Glover tower is not only the best denitrificator, but a most economical concentrator of chamber acids, and the most desirable point in the circuit at which to add the nitre.

Full descriptions are given of Faur & Kessler composite platinum and lead stills, and of the modifications in shape, with consequent reduction in weight, which this innovation has driven the old platinum-still makers to adopt.

All the volume needs is an index, which, strange to say, is altogether wanting. J. DOUGLASS.

9. *The Chemistry of Common Life;* by the late JAMES F. W. JOHNSTON, Professor of Chemistry in the University of Durham, etc. A new edition, revised and brought down to the present time by A. H. CHURCH, M.A. 592 pp., 8vo. New York, 1880. (D. Appleton and Co).—A great advance has been made since the publication of the first edition of this work, by Professor Johnston, twenty-five years ago, both in the science of Chemistry itself and in the extent to which the general public is familiar with its principles. The novelty of the discussions is consequently not now so great as then, but on the other hand the general interest felt in them is probably greater. This clear exposition, by Professor Johnston, of the many points in which Chemistry touches

every-day life, has been and will still be, the means of doing much good. The editor, Professor Church, has brought the book down to the present time, but throughout, as he states, with the design to respect both the method and style of the original work.

10. "*Why the Air at the Equator is not Hotter in January than in July;*" by A. Woeikof (St. Petersburg).—In Nature, vol. xxi, p. 129,* Mr. Croll gives his reasons why the equator is not much warmer in January than in July, notwithstanding the greater nearness of the sun at the former season. To state the case briefly, he, having recalled the fact that the whole earth is colder in January than in July, because in the former the cold winter of the northern (or principally land) hemisphere coincides with the mild winter of the southern (or principally water) hemisphere, he continues: "Consequently the air which the equatorial regions receive from the trades must have a higher temperature in July than in January. The northern is the dominant hemisphere; it pours in hot air in July and cold air in January, and this effect is not counterbalanced by the air from the opposite hemisphere. The mean temperature of the air passing into the equatorial regions ought therefore to be much higher in July than in January, and this it no doubt would be were it not for the counteracting effects of eccentricity." And further: "There is another case which must also tend to lower the January and raise the July temperature of the equator: the northern trades pass farther south, and consequently cool the equatorial regions more during the former than the latter season."

I maintain that there is no such influence of the northern trades on the temperature of the equator, because they scarcely anywhere reach it, and then because the lower latitudes of the northern hemisphere are not colder in January than those of the southern hemisphere in July. *In the Atlantic the northern trades do not reach the equator at all in January*, but only in February, March and April, and this but in the western part of the ocean. The same may be said of the Pacific in its eastern part, where alone the trades are regular. In the Western Pacific, as well as in the Western Indian Ocean, I admit that air from the northern hemisphere reaches to the equator and somewhat beyond in January but not that this tends to give the equator a lower temperature in this month than in July. According to Dove, the mean temperature of 10° N. in January is 77°·2; of 10° S. in July, 76°·1.

So far as the temperature of the equator is concerned, the southern is the dominant hemisphere, and the equator is certainly cooled by winds coming from the south. If the equator is not everywhere warmer in January than in July, this is caused by the rainy season, which on the equator, and even a few degrees north of it, generally coincides with the southern summer. Where the rains are not very heavy, as, for example, on the Isle of St. Thomas, West Africa, we have: January, 78°·3; July, 75°·7; at Padang, Sumatra, where the rains are exceedingly heavy all the

* This Journal, February, 1880, p. 142.

year, there is scarcely any difference at all between the months. Somewhat to the south of the equator, where to the difference in the nearness of the sun is added a much greater height above the horizon in January, we have—

	January.	July.	Rainy Season.
Amboina, Molucca Islands, 4° S.	80°·9	77°·4	May to August.
Batavia, Java, 6° S.	77°·7	78°·8	December to February.
Pernambuco, Brazil, 8° S.	80°·6	75°·0	April to July.

so, by the first-rate observations of Batavia, it is established that, so far as 6° S., January is 1°·1 colder than July, because the former is very rainy, while the latter has little rain. Even to 9° lat. N., July is colder than January, if the former has much more rain, so for example—

Fernando Po, W. Africa, 4° N.	79°·9	76°·5	March to November.
Gondokoro, Upper Nile, 5° N.	81°·3	75°·7	April to August.
Freetown, W. Africa, 8½° N.	80°·4	77°·0	June to October.

Thus, in the lowest latitudes of the northern hemisphere, we find differences amounting to 5°·6, while in the southern greater differences than 1°·1 are not known, which may, to a certain degree, be ascribed to the nearness of the sun in January.

I think I have proved that, as to what we call the temperature of the air (really that of the lowest stratum), it is, on the equator and a few degrees north and south from it, far more influenced by the yearly distribution of clouds and rain than by the different amount of heat received from the sun. The result would be different if we knew the temperature of the whole stratum of air. The heating of the upper surface of the clouds by the sun, and especially the heat liberated in the condensation of water must give to the higher strata a superior temperature than that they had in the dry season; in other words, the decrease of temperature with elevation is much slower during the rains than in the dry season, as was shown for India by Mr. Blanford. This is true for other regions, and where the sky is cloudy and rains abundant in the greater part of the year, the temperature of the whole air may yet be higher than in drier climates, where the soil and the lower stratum of air are hotter.

I do not agree with Mr. Croll in what he states at the end of his letter as to the effect of winds in cooling the equatorial regions and rendering them habitable, as they would be too hot for man without the cool air brought from the temperate regions. I think Mr. Croll has enormously over-stated the effects of winds on the temperature of the equator. The extent of the tropical zone is so great, its temperature so very near to that of the equator, the winds which blow across it so gentle, that I consider the effect of the winds from the temperate regions in directly cooling the temperature of the equator to be nearly imperceptible. The following is a good illustration:—Nowhere is the winter temperature so low near the tropics as in Southern China, for example, in January, Canton, 55°·6, Victoria, Hong-Kong, 59°·2. Yet Saigon, in Cochin China, but 11° to the south of Hong-Kong, and subjected

to the full force of the northeast monsoon from the China seas, has a January temperature above 77°. Clearly the thermal effect even of the cold winter monsoon is scarcely perceptible farther south.

I consider water to be the only direct cause of the mildness and uniformity of equatorial temperatures, and this in three ways—(1) by the great heat-capacity of water; (2) by the clouds which interpose a screen between the sun and the surface of the earth; (3) by the evaporation of rain-water by the soil and plants.

The first cause is especially powerful on the ocean, while the two latter act especially on land, even very far from the sea. If it was not for the clouds and evaporation, how could we explain, for example, the absence of great heat (hottest month, 78°·6) at Iquitos, on the Amazons, 4° S., and more than 1,000 miles from the Atlantic, where the winds are generally weak?

As to the winds, I admit of their effect in this case; but (1) in causing ocean currents, and thus removing the heated water from the equator; (2) in spreading the cold air from over the cold currents over a greater distance. The latter is the cause of the low temperature in the equatorial regions of the Eastern Atlantic and Eastern Pacific.

Where the sky is clear and humidity and rains deficient, very high temperatures of the air are attained, even at a great distance from the equator (10°–30°) and this notwithstanding winds of considerable force blowing from cooler regions. So, for example, the north winds blowing in the summer in the Sahara, and coming from the cooler Mediterranean, are certainly stronger than the trades of the ocean and yet do not prevent the desert from attaining a higher temperature than known in any equatorial region.—*Nature*, Jan. 15.

11. *Report on Magnetic Determinations in Missouri in the Summer of* 1879; by FRANCIS E. NIPHER.—The magnetic survey of Missouri, commenced in 1878, was continued during the summer of 1879. Observations of the declination and inclination of the needle, and of the horizontal intensity, were made at a considerable number of new stations. The report gives the methods employed and the results of the observations in detail. A map is added giving the isogonic lines for both Missouri and Iowa, those of the latter State being based upon the survey by Dr. G. Hinrichs. These isogonic lines exhibit remarkable flexures, which are believed to bear an intimate relation to the drainage systems of the two States. Prof. Nipher offers the following explanation:

Assuming the existence of earth-currents of electricity, the general direction of which is from east to west, they distribute according to well-known laws, flowing in greatest quantity through the lines of least resistance. The magnetic needle tends to set at right angles to the current, following the well-known law enunciated by Ampère. Where the general direction of the moist river valley is at right angles to the normal position of the magnetic needle, the position of the latter is not changed, as the tendency of the needle is to set at right angles to a current of elec-

tricity. In such a valley (as, for instance, in the Missouri valley between Jefferson City and the mouth of the river), the direction of the needle should be normal, the direction in which the water flows being without (appreciable) effect. Where the river runs at right angles to the earth currents, the disturbing cause would be practically removed, as there would be no deflected component of the earth current along the river valley.

The maximum effect is produced in the case of rivers making an angle of 45 degs. with the general direction of the earth currents. Of course, this effect would be most marked where the river and valley are very large, as in the case of the Mississippi and Missouri, or where we have an immense drainage system consisting of long rivers and creeks running parallel and in the proper direction, as is the case on the eastern slope of Iowa.

Whether the explanation above suggested be the true one or not, the deflection of the 11° and 10° lines to the east in the western part of Iowa, the abrupt bends between St. Joseph and Kansas City and between Glasgow and Jefferson City, the westward bending of the 8° and 8° 30′ lines in the Osage Valley as compared with the 9° line in the same latitude and the remarkable flexures in eastern Iowa, are all in harmony with it.

12. *Variations in the Magnetic Declination deduced from observations made at Moncalieri (Piedmont) from* 1871–78.—The conclusions deduced by R. P. Fr. Denza in relation to the variations of the magnetic declination in Piedmont are as follows: The mean monthly variation of the declination attains a minimum in December. It increases at first slowly, from December to February, and then more rapidly from February to April. The largest values are attained in April and June, the first of which is a little the larger. In the intermediate month of May there is a sensible diminution of the value. After attaining the maximum in June there is a second diminution during July and August slowly, and more rapidly in the autumn months to December.—*Comptes Rendus*, Jan. 12.

13. *On a new action of Magnets on Electric Currents;* by E. H. Hall.—In a letter from the author, received by the editors since the article on pp. 200–205 was printed, he states that the values of *M*, the strength of the magnetic field, given in the article should be multiplied by a constant factor which is nearly 2. The precise value of this reduction factor cannot be given, but the change does not affect the main conclusions arrived at. Mr. Hall adds that he hopes in a month or two to publish more exact numerical results in regard to the new action as observed in several different metals.

II. Geology and Mineralogy.

1. *Geology of the Rio São Francisco, Brazil.*—Mr. O. A. Derby, of the Brazilian National Museum, has recently been making, in company with a party of government engineers, a geological examination of the Rio São Francisco, from the falls of Paulo Affonso, to Januaria. From the latter place Mr. Derby was to proceed to Rio de Janeiro overland, by way of the rich mineral districts of Minas Geraes. The latest news from Mr. Derby is contained in a letter, dated near the city of Barra, on the São Francisco, November 9, 1879, which gives us a few general statements of interest, as to the result of his observations up to that time. From Paulo Affonso, the ascent of the river for eighty leagues was made in canoes, and then a small steamer was procured to accomplish the remainder of the journey. Cretaceous fossils, similar in character to those of the fresh-water basin of Bahia, were discovered in abundance, in the sandstone formation about Paulo Affonso. Leaving the sandstone region, they traveled for a long distance through a gneiss region, with occasional patches of itacolumite. This was succeeded by a region, composed mostly of itacolumite, which extended as far as they had gone; but two days out from the city of Barra, they came upon the horizontal limestones of the upper river, which Mr. Derby proposed to study with great care, in order to determine their geological age. So far they had yielded no fossils, but Mr. Derby was hopeful of finding at least some remains, as the beds are very well developed in certain localities, yet remaining to be visited. In closing, Mr. Derby remarks: "One thing is certain, we have got to give up the great Tertiary depression and greatly extend the area of the Cretaceous. I find that many of the beds, which have been described as horizontal and undisturbed, have really suffered upheaval, and are far from horizontal over large areas." R.

2. *Age of the Taconic rocks and Geology of Vermont, according to Professor C. H. Hitchcock.*—Professors Edward and C. H. Hitchcock, in describing the Taconic system (the slates, limestones, quartzyte, etc.) in the Geological Report of Vermont, after treating of its distribution, its rocks and its fossils, and apparently referring it on account of the fossils to the Lower Silurian, have, next, a closing section (on pages 446, 447) headed *Presumptions in favor of the Taconic System*, and in this section the conclusion is formally stated that the Taconic System underlies the Lower Silurian. I was led, therefore, in preparing the bibliographical note published on page 153 of this volume (and also in the new edition of my Manual of Geology, page 835) to place the Vermont Report with those works that make the Taconic beds pre-Silurian, as I had previously done when writing my articles on Vermont geology. In a letter from Professor C. H. Hitchcock, dated February 10th, he states that the "Presumptions" were introduced "as a brief exposé of the Taconic System, couched in such

language as Emmons himself would have used." It is with great satisfaction that I am able to make the correction and cite, from the same letter, the fact that "there is nothing in the Report anywhere favorable to Taconism," although, as he also writes, it does not refer the system distinctly to the Lower Silurian, except in the statement of paleontological facts that really support this view. On the earlier pages, 251 to 257, Professor Edward Hitchcock gives a section through the Taconic region and views as to the folds which accord fully with this opinion.

He says also that at the time when the fossils were discovered and submitted to Professor James Hall, just before the publication of the Report, it was Mr. Hall's opinion that the quartzyte was the Medina sandstone, and that the Taconic group was mostly above the Lower Silurian, and hence the reference of some of the fossils—none of them very distinct specimens—to the Upper Silurian.

Professor Hitchcock also says, in the recent letter to me, after remarking on his disbelief in "Taconism:" "Within the past two years I have gone over most of the Vermont sections, and have felt that they demonstrated the essential equivalence of the Taconic system with the Potsdam and the overlying limestones and slates [of the Lower Silurian]. I have been throughout in essential accord with you and Mr. Wing." He adds that Mr. Wing's views had been his for years.

This important correction was not received until after my article on the age of the Green Mountains (p. 191) was printed, or else this note would have been attached to it. J. D. DANA.

3. *A Monograph of the Silurian Fossils of the Girvan District in Ayrshire;* with special reference to those contained in the "Gray Collection," by H. ALLEYNE NICHOLSON and ROBERT ETHERIDGE, Jr. Fasciculus II. Trilobita, Phyllopoda, Cirripedia, and Ostracoda, pp. 137–233, with plates X–XV. Edinburgh and London: 1879. Wm. Blackwood & Sons.

4. *On Spodumene and its Alterations, from the granite veins of Hampshire County, Massachusetts.*—Mr. A. A. JULIEN has published recently (Annals N. Y. Acad. Sci., Nov., 1879), a valuable and extended memoir on spodumene and the results of its alterations. The two localities which are particularly described are those of Goshen and Chesterfield, Massachusetts. Analyses of pure and unaltered spodumene from these localities yielded the results given below. No. 1 was from the Levi Barrus farm in Goshen; specific gravity = 3·19. No. 2 from Chesterfield Hollow; specific gravity = 3·185 and 3·201.

	SiO_2	Al_2O_3	Fe_2O_3	MnO	MgO	CaO	Li_2O	Na_2O	K_2O	H_2O	
I	63·27	23·73	1·17	0·64	2·02	0·11	6·89	0·99	1·45	0·36	= 100·63
II	61·86	23·43	2·73	1·04	1·55	0·79	6·99	0·50	1·33	0·46	= 100·68

These closely agreeing analyses correspond very nearly to the formula $Li_2Al_2Si_4O_{12}$, for which the quantivalent (= oxygen) ratio

for $\overset{I}{R} : \overset{VI}{R} : \overset{IV}{Si} = 1 : 3 : 8$. This is the same formula recently deduced by Dœlter (see this Journal, xvii, 333, 1879), and though differing from the one before accepted, agrees with that obtained by Brush in an early analysis of the Norwich (now called Huntington) spodumene in 1850 (l. c. II, x, 370). The true composition of spodumene may consequently be accepted as established beyond a doubt. Mr. Julien remarks very justly that the specimens previously analyzed had doubtless suffered partial alteration, bringing with it a duller color and an inferior luster, translucency, hardness and specific gravity.

Much of the spodumene from the two localities mentioned, is altered into *cymatolite*. This name was given to a similar mineral from Goshen by Shepard, but the imperfect analysis published, with later that of Burton, left doubt as to the true composition of the species. This point has been very satisfactorily determined by Julien. The Goshen variety of cymatolite, previously called *aglaite* by the same author, occurs only as a continuation of the square prisms of spodumene, sometimes six or eight inches long, not as a coating over them. The structure is micaceous, the lamination flat, rarely undulating, and always in the plane of the orthodiagonal cleavage of the original spodumene. The laminæ are brittle but the thinner scales are flexible, somewhat elastic and transparent; they often project slightly beyond the sides of the crystal. The physical characters are: luster silvery to satin; color white; feel soft; hardness = 1·5; specific gravity = 2·753.

The Chesterfield variety is much more abundant. It occurs forming the whole, or with the original mineral a part, of crystals of enormous size; one is mentioned which was 35 inches in length while still lying in the vein and with a diameter of 10 to 11 inches. The structure is intermediate between micaceous and fibrous, with a strong wavy tendency of the foliation, on the surface of fracture. In the smaller crystals the plane of foliation is usually at right angles to the faces of the spodumene, and the folia therefore radiate from a central plane in the crystals which are completely altered; but in the larger ones, within a thin radiating crust of this kind, the folia generally conform to the central plane of cleavage of the spodumene and a parallel foliation often results. Sometimes a core of blackish-green pinite (killinite) is found in the smaller crystals; while in the larger crystals the core consists of spodumene often with killinite as a thin layer next to the white crust. A greenish-yellow muscovite is commonly intercrystallized in the larger pseudomorphs in scales or films. The hardness = 1·5–2; specific gravity = 2·696–2·700; laminæ brittle. The following are analyses of cymatolite; 1, from the Manning farm and 2 from the Barrus farm, both in Goshen; 3 from Chesterfield Hollow.

	SiO_2	Al_2O_3	Fe_2O_3	MnO	MgO	CaO	Li_2O	Na_2O	K_2O	H_2O	CoO	
1.	58·51	21·80	0·85	0·29	1·44	0·84	0·19	6·88	6·68	2·40[1]	*tr*	=99·88
2. Aglaite	58·11	24·38	1·66	0·18	0·75	0·48	0·09	2·57	8·38	3·01[2]		=99·61
3. $\frac{3}{2}$	58·58	22·28	1·77	0·15	0·45	0·93	0·10	9·08	4·48	2·08		=99·90

[1] With nitrogenous organic matter 0·44. [2] do 0·43.

Two other analyses are also given; they agree closely with those here quoted, varying only as do these in the amounts of soda and potash. The calculated formula is $H_2(H, Na, K)_6Al_6Si_{12}O_{37}$, which requires SiO_2 58·46, Al_2O_3 24·99, Na_2O 15·09, H_2O 1·46 = 100. It is shown that the formula is the same as that of spodumene except in the protoxide elements present and the additional molecule of water. Mr. Julien also describes in detail the microscopic characters of cymatolite, and from the fact of its greater atomic volume as compared with spodumene, argues that the process of alteration must have been accompanied with the exertion of a great pressure, the results of which are believed to be observed in many cases in the distortion of the pseudomorphs.

As mentioned above the variety of pinite called *killinite* also occurs as a pseudomorph after spodumene, though more sparingly than cymatolite. It has a foliated texture, retaining the cleavage of the original mineral. Hardness = 3·5, sp. gr. = 2·623–2·652. Luster dull and greasy to vitreous, the latter on the cleavage planes. Color greenish-gray to olive-green, also greenish-black; feel greasy. An analysis of the mineral from Chesterfield Hollow (G. = 2·623) yielded

SiO_2	Al_2O_3	FeO	MnO	CoO	MgO	CaO	Li_2O	Na_2O	K_2O	H_2O
46·80	32·52	2·33	0·04	0·04	0·48	0·77	0·32	0·78	7·24	7·66

Organic matter 1·14 = 100·12.

For this the formula $H_6K_2Al_4Si_5O_{20}$ is obtained or $H_2K_2Al_4Si_5O_{18}+2aq$. The original killinite is from Killiney Bay in Ireland; it corresponds closely with the Chesterfield mineral.

In addition to the above pseudomorphs, others after spodumene are described which consist:—(1) of vein *granite*, made up of muscovite, albite and quartz with large cymatolite columns passing from one end to the other of the mixture forming masses one hundred pounds or more in weight; (2) of greenish-yellow *muscovite*, more or less intermixed with cymatolite, it sometimes occurring only in minute, disseminated scales, and in others making up the whole of the pseudomorphous crystal, retaining the form and striations of the spodumene. These and the intermediate varieties are regarded as the results of intercrystallization of the two minerals, in the process of alteration: (3) of *albite*, generally intermixed with muscovite and quartz: (4) of *quartz*; these pseudomorphs are rare, and while retaining the form of the original mineral contain more or less mica. The last two forms are mentioned as varieties of (1) above.

Mr. Julien closes his very interesting paper with some remarks on the paragenesis of spodumene and the character of the alteration which has resulted in the formation of the pseudomorphs described. Two figures illustrate the relations of the several species.

5. *Crystals of Wollastonite.*—Professor O. Root mentions the discovery of very large crystals of wollastonite at Diana, New York; one crystal was upwards of eleven inches in length and ten inches across the prism, both terminations being complete. He has also obtained unusually large and perfect crystals of mica from the locality on the west bank of Vrooman Lake, N. Y.

III. Zoology.

1. *Fresh-water Rhizopods of North America;* by Joseph Leidy, M.D., Prof. Anat. Univ. Pennsylvania. 324 pp. 4to., with 48 colored plates. Washington, 1879. Vol. xii of the Reports of the U. S. Geological Survey of the Territories, F. V. Hayden, Geologist-in-Charge: Department of the Interior.—This new work, by Dr. Leidy, is a very important addition to the quarto series of reports connected with the Geological Survey of the Territories under Dr. Hayden. It is the result of a vast amount of careful microscopic research with regard to the structure, development and habits of these lowest forms of animal life over the North American Continent; and the numerous plates with crowded colored figures are attractive for their beauty, as well as for the instruction they impart.

Dr. Leidy shows in his descriptions and faithful delineations that he almost loved the little things, the study of which gave him so much pleasure. "In his concluding remarks," he says:—

"The objects of my work have appeared to me so beautiful as represented in the illustrations, and so interesting as indicated in their history which forms the accompanying text, that I am led to hope the work may be an incentive, especially to my young countrymen, to enter into similar pursuits. * * * 'Going fishing?' How often the question has been asked by acquaintances as they have met me, with rod and basket, on an excursion after materials for microscopic study. 'Yes,' has been the invariable answer, for it saved much detention and explanation; and, now, behold, I offer them the results of that fishing. No fish for the stomach, but as the old French microscopist, Joblet, observed, 'some of the most remarkable fishes that have ever been seen;' and food fishes for the intellect."

These fresh-water Rhizopods are of special interest to the philosopher, as well as the naturalist, because they belong to the lowest division of the animal kingdom—the higher section of the Protista of Hæckel—and yet are very decidedly animal in their characteristics, and wonderfully complex in their animal functions. As Leidy states (p. 5):

"The soft mass of protoplasm, or sarcode, forming the essential part of all Rhizopods, has no internal cavity like the body-cavity of higher animals, neither has it a mouth like the higher Protozoa, nor has it stomach or intestine. Without trace of nerve elements, and without definite, fixed organs of any kind, internal or external, the Rhizopod—simplest of all animals, a mere jelly speck—moves about with the apparent purposes of more complex creatures. It selects and swallows its appropriate food, digests it and rejects the insoluble remains. It grows and reproduces its kind. It evolves a wonderful variety of distinctive forms, often of the utmost beauty; and indeed, it altogether exhibits such marvelous attributes, that one is led to ask the question in what consists the superiority of animals usually regarded as much higher in the scale of life."

Dr. Leidy divides the Rhizopods into five orders: I. *Protoplasta*, II. *Heliozoa*, III. *Radiolaria*, IV. *Foraminifera*, and V. *Monera;* agreeing in this with the views of Professor L. E. Schulze, as brought out in a recent Number of the Archiv für Mikroscopische Anatomie (1877, p. 21). The Protoplasta and the Monera correspond to Hæckel's Protista. Excepting a few of the Monera section, fresh-water Rhizopods belong almost entirely to the Protoplasta and Heliozoa. All the fresh-water species described by Leidy are of these two groups, excepting one Foraminifer, named by Leidy *Gromia terricola*, the genus *Gromia* being exceptional among Foraminifera in that it is represented by several species inhabiting both salt and fresh water. The Protoplasta include the genera *Amœba*, *Difflugia*, *Nebela*, *Arcella*, and others; and the Heliozoa, *Actinophrys*, *Heterophrys* and others allied. With regard to Monera he says, "though Professor Hæckel has indicated and described a number of fresh-water species, I am not sure that I have had the opportunity of finding any of them, excepting perhaps the genus *Vampyrella* of Cienkowski, which he ascribes to the same order."

Dr. Leidy's experience enables him to give important information as to the localities of these species, and the best methods of collecting them. The following paragraphs taken from pages 8 to 11, are a part of his observations on these subjects:

"Fresh-water Rhizopods are to be found almost everywhere in positions kept continuously damp or wet, and not too much shaded. They are especially frequent and abundant in comparatively quiet waters; clear, and neither too cold, nor too much heated by the sun, such as lakes, ponds, ditches, and pools. They are also frequent in wet bogs and savannas, among mosses, in springy places, on dripping rocks, the vicinity of waterfalls, springs, and fountains, and in marshes, wherever the ground is sufficiently damp or moist to promote the growth of algæ. They are also to be found in damp shaded places, among algæ, liverworts and mosses, about the roots of sedges, rushes and grasses, or those of shrubs and trees growing in or at the borders of bogs and ponds or along ditches and sluggish watercourses. They are likewise to be found with algæ in damp shaded positions in the depressions and fissures of rocks, in the mouths of caves, among decaying logs, among mossses and lichens, on the bark of growing trees, and even in the crevices of walls and pavements about old dwellings and in cities.

"The favorite habitation of many kinds of Rhizopods is the light superficial ooze at the bottom of still waters, where they live in association with diatoms, desmids, and other minute algæ, which form the chief food of most of these little creatures. They never penetrate into the deeper and usually black mud, which indeed is almost universally devoid of life of any kind.

"Rhizopods also occur in the flocculent materials and slimy matter adherent to most submerged objects, such as rocks, the dead boughs of trees, and the stems and leaves of aquatic plants.

A frequent position is the under side of floating leaves, such as those of the Pond-lily, *Nymphæa odorata;* the Spatter-dock, *Nuphar advena;* and the Nelumbo, *Nelumbium luteum.* Certain kinds of Rhizopods, especially the Heliozoa, or Sun-animalcules, are most frequent among floating plants, such as Duck-meat, *Lemna;* Hornwort, *Ceratophyllum;* Bladderwort, *Utricularia;* and the various Confervas, as *Zygnema, Spirogyra, Oscillatoria,* and the Water-purse, *Hydrodictyon.*

"In no other position have I found Rhizopods of the kind under consideration in such profusion, number, and beauty of form as in sphagnous bogs, living in the moist or wet Bog-moss, or *Sphagnum.* Sometimes I have found this particular moss actually to swarm with multitudes of these creatures of the most extraordinary kinds and in the most highly developed condition. A drop of water squeezed from a little pinch of Bog-moss has often yielded scores of half a dozen genera and a greater number of species. Frequently, however, the Sphagnum of many localities contains comparatively few Rhizopods, though I have rarely found them entirely absent from this moss. Other mosses and liverworts I have not observed to be specially favorite habitations of the Rhizopods, not even such aquatic kinds as the *Fontinalis.*"

In water squeezed into a watch crystal from a small bunch of Sphagnum Dr. Leidy obtained thirty-eight species.

"The mode I have habitually adopted for collecting Rhizopods, which is also equally well adapted for collecting many other microscopic organisms, plants, and animals, is as follows:

For ponds, ditches, or other waters, I use a small tin ladle, or dipper, such as is commonly employed for domestic purposes. Into the handle I insert a stick of convenient length, and for this I usually carry with me a jointed pole of two or three pieces, each about five feet. The dipper is used by slowly skimming the edge along the bottom of the water so as to take up only the most superficial portion of the ooze, which is then gently raised from the water and transferred to a glass jar. A small hole in the bottom of the ladle favors the retention of the collected material, but care should be taken that it is not so large as to permit the material to stream through. After the collecting-jar is full, if more of the material is wanted, after allowing that in the bottle to settle, I pour off a portion of the water and supply an additional quantity from the locality.

"Usually, I have proved more successful in obtaining Rhizopods from the ooze near the shores of lakes and ponds than I have in deeper water; but this I suspect was mainly due to the circumstance that near the shore I could see the ooze at the bottom of the water, and could much better manage to collect the desired material.

"Aquatic plants, if rooted in the mud, should be carefully cut off and gently lifted from the water so as to disturb as little as possible the adherent materials. A sufficient quantity being placed in a tin preserving-can or other vessel, water from other portions of the plants may be squeezed upon that which is retained.

"Wet Sphagnum may be collected and put in tin preserving-cans, and the water of other portions may be squeezed upon the portion preserved. The same process may be pursued with other mosses.

"From the surface of the ground in wet places, to collect the Rhizopods, it is sufficient to scrape up, with the broad blade of a knife, the green algous material with which the animals are usually associated."

With regard to localities of marine Foraminifera along coast regions, he remarks, p. 17:

"Sea-sands contain as an important constituent the dead shells of recent Foraminifera, though in very variable proportions. They are generally most abundant in the sands of warmer latitudes, and especially on shores profusely furnished with sea-weeds.

"Plancus,* who, according to D'Orbigny, was the first to describe and figure the shells of Foraminifera, counted 6000 individuals in an ounce of sand from the Adriatic. D'Orbigny estimated that there were 160,000 in a gram of selected sand from the Antilles. Schultze gives 1,500,000 as the number he found in fifteen grams of sand from Gaeta on the coast of Sicily.

"Even on the comparatively barren shores of New Jersey, consisting of quartz sand, foraminiferous shells occur in notable quantity. In a portion scraped from the surface between tides, at Atlantic City, I estimated that there were 18,700 shells to the ounce avoirdupois, all of a single species of *Nonionina*. In another sample, from Cape May, I obtained 38,400 shells to the ounce, likewise of the one species.

"In sand collected by scraping up the long white lines on the bathing beach at Newport, Rhode Island, occupying an indenture of the rocky coast, covered with sea-weeds, foraminiferous shells were found to be much more numerous, but, excepting in the case of some examples of Miliola, of smaller size. In an ounce of the sand, I estimated that there were about 280,000 shells, of several genera and species."

One of the most remarkable forms described in the book is the *Dinamœba mirabilis*, from the Cedar swamps of New Jersey, represented by many figures on plates 6 and 7. It is commonly cream-white or greenish-white in color, but spotted often with green, brown, and yellow, all the colors, excepting the white, being due to the food-balls, which are chiefly the Desmids, *Didymoprium* and *Bambusina*. It is a gluttonous feeder, and is commonly so gorged with this vegetable food as to be more or less opaque. Every part of the surface, including the pseudopods and posterior papillæ, is ordinarily bristled with exceedingly minute spicules or ridged cils; but after some hours these may disappear or be represented by minute molecules. A still more remarkable feature is the occurrence of a thick investment of hyaline jelly, the outer surface of which is defined by innumerable, exceedingly

* Ariminensis de conchis minus notis. Venice, 1739.

minute rods, standing perpendicularly, which make the animal look as if surrounded by a nimbus of Bacteria. "In the movements of Dinamœba, its jelly-like cloak appears to be no obstacle, and the subulate pseudopods shoot through and beyond it as if it did not exist."

Another species of peculiar interest is *Hyalosphenia papilio*, a buff-colored, or straw-colored species, perfectly transparent, and remarkably constant in its form.

The species of Nebela, particularly *N. collaris*, *N. hippocrepis* and *N. ansata*, are of special beauty; but we must refer to the work with its plates, for the facts respecting these and the various other kinds. The book is adapted to the uninitiated as well as to adepts in the science. Dr. Leidy says, "In the course of its preparation, I have always had my pupils in mind, and I shall be glad if it serves as an additional aid to their studies;" and we add that it is well adapted to this and its higher purpose.

The work closes with a Bibliographic appendix, containing the names of authors of works and memoirs on living Rhizopods and lists of all the species they describe, together with the synonymy so far as giving the names of the same adopted by him.

2. *Zoology for Students and General Readers;* by A. S. PACKARD, Jr. 8vo, 719 pp. 544 cuts. New York: 1879. (Henry Holt & Co.)—This work is one of the best of the various manuals of Zoology that have recently appeared, and is decidedly better adapted for use in the class-room and laboratory than most of them. The general treatment of the subject is good, and the descriptions of structure and the definitions of groups are for the most part clear, concise, and not so much overburdened by technical terms as in several other manuals of structural Zoology now in use. The present work is largely devoted to structural or morphological Zoology, with pretty full accounts of the embryology of the various groups. Somewhat detailed accounts of the anatomy of various common representatives of the more prominent groups, both of vertebrates and invertebrates, add much to the value of the work for laboratory instruction. These are illustrated by good, original figures, showing the more prominent anatomical features. The dissections of vertebrates, and the figures illustrating them, are by Dr. C. S. Minot. The illustrations are throughout copious, and generally good and well-selected, though mostly borrowed from other works.

The classification adopted is, for the most part, nearly in accordance with the more recent European writers, and not very different from that of Huxley's recent works. One feature, that of dividing the "Crustacea" into two great groups, *Neocarida* and *Palæocarida*, is of very doubtful utility. If such a division be necessary it would seem better to adopt the name Crustacea for the former group, as has been done by others who have proposed the same division (under the name *Merostomata*), and to have given a new name (if any be needed where several are in use) only to the group cut off from the Crustacea. But it is doubtful whether

the *Palæocarida*, including, as it does, Trilobites and Limuloids, can be maintained, with our present knowledge of the former, as a natural group, while it has been fully shown by the anatomical researches of A. Milne Edwards and others that *Limulus* is not a Crustacean, in any proper sense. The *Pycnogonida* have also been badly treated, for this group, remarkable for so many anatomical and morphological peculiarities is dismissed in three lines (p. 360), as a family of *mites!* But the *Pycnogonida* would not, by any means, go under the definition of the sub-class *Arachnida*, much less into the order *Acarina*, for many of them have a larger number of limbs than any true *Arachnida*. The dilation and relaxation of the limits and definitions of Insecta so as to include, not only the Arachnida, and Pycnogonida, but also *Peripatus*, seem to us objectionable. The introduction of Mollusca between the Vermes (including *Annelida*) and the Arthropoda does injustice to the exceedingly close relationship existing between the Annelida and lower forms of Crustacea and Insecta; but others have done so before. In the present unsettled condition of zoological classification, it would be useless, however, to lay much stress upon the particular views adopted by any writer, for these views are continually changing, as discovery advances.

A few errors, mostly of no great importance, we have noted. Doubtless the author will, at an early date, have an opportunity to correct them in a second edition. A few of the figures are incorrectly named: thus, fig. 74 represents *Asterias Forbesii* (not *A. vulgaris*); fig. 225, is *Palæmonetes vulgaris* (not *Crangon vulgaris*). On page 60, *Sarsia prolifera* is mentioned as "the only example known of budding in free medusæ," the author evidently forgetting several New England species that are well known to have this peculiarity in a marked degree and have long ago been so described in the works of L. Agassiz, A. Agassiz, and others. *Hybocodon prolifer* Ag., and *Dysmorphosa fulgurans* are notable examples. The statement on p. 390 that "the products of digestion do not pass through the walls of the stomach and directly enter the circulation, as in invertebrates," is an obvious error, unless profoundly modified, by putting a small part for the whole. There appears to be some confusion on pp. 418 and 420 in reference to the breeding of the "dogfish," for on the former page it is said that they lay eggs, while on the latter page "the dog-fish (*Squalus Americanus*)" is mentioned. The latter produces living young, as well as the *Mustelus canis*. A. E. V.

3. *Das System der Medusen* (*Erste Hälfte des ersten Theils: System der Craspedoten*); von Dr. ERNST HÆCKEL. i–x and 360 pp., with 20 plates. Jena, 1879.—Since the publication of Eschscholtz's System der Akalephen, an immense number of additions to our knowledge of some of the smaller groups of Medusæ have been made. The principal attempts to revise the classification of the group as a whole we owe to Gegenbaur (1856) and Agassiz (1862). Their own observations were based either upon European or American species, and they could do but little toward

clearing up the relationship of the many Acalephs described by older writers in the great voyages of circumnavigation of the earlier part of this century. Up to the time of Gegenbaur and Agassiz a great number of young forms of free Medusæ had been described, either as new genera or new species entirely independently of the study of the Hydroids and of their developmental history. It is easy to see that endless confusion must little by little have crept into the classification of the group. To reëstablish under these circumstances a certain amount of order in the classification of Acalephs, an investigator was demanded thoroughly familiar with the Acalephs of several districts in their living condition. Agassiz and Gegenbaur, though often holding very dissimilar views, have greatly simplified the classification of Medusæ, but no one has taken up the general subject since their time, and innumerable papers on special points of classification, anatomy and embryology have been published. Hæckel's System der Medusen, of which the first part is issued, is intended to incorporate all this material. With his extensive knowledge of Acalephs obtained during frequent visits to different points of the seashore, he proposes to revise the whole group, adding to the species already determined a great number of new species, nearly all of which are very effectively illustrated.

It is very satisfactory to find this work of Hæckel's free from the abusive personalities which have disgraced so many of his more recent productions, and to find his investigations full of ingenious views, acute criticisms, and speculations based upon observations and not upon fanciful theories. This memoir will take its place beside his Monographs on Radiolaria, on Sponges, and on the development of Acalephs, which have given Hæckel so prominent a place among investigators.

It seems unfortunate that Hæckel's facility for coining new names should lead him to reject so frequently the established names of the higher sub-divisions whenever they do not have the identical limits he himself assigns to any group. This method carried to its logical conclusion will render the rejection of all accepted names which do not enter into the systematic or morphological views of an author, not only necessary but imperative. Hæckel has no patience with systematists who constantly replace old names and compel the writers of the present day not to ignore completely their predecessors. He accuses them of needlessly increasing the existing confusion. Yet he himself ignores as completely existing nomenclature, and in his zeal to adopt or coin names representing his individual views, introduces a far greater confusion. This extreme method is not limited with Hæckel to the higher groups, but extends systematically to families, subfamilies and even genera, so that it will hereafter often be extremely difficult to trace the history of a genus or species which Hæckel has removed from one place to another as arbitrarily as the very systematist whom he so often takes to task. The existing confusion which is so frequently his theme is indeed only increased by his own equally arbitrary proceedings.

We certainly do not seem to gain anything either in exactness or in our knowledge of the groups by having the Tubularians (in the widest sense) appear again as Anthomedusæ or the Campanularians as Leptomedusæ. Yet in the primary sub-divisions of the Craspedota the classification adopted by Hæckel is an advance upon previous ones. The position of the Trachynemidæ and Cuninæ has always been a doubtful one. Hæckel enters the field with a mass of new information, and the position he assigns these groups is well sustained by the evidence he advances. He has likewise from the great number of Geryonidæ he has himself examined, finally cleared up the chaos existing in regard to the relationship of the family, and the divisions he adopts are extremely satisfactory. The recent magnificent histological work of the Hertwigs, of Eimer and others has thrown a flood of light on the affinities of many of the primary groups of Medusæ, of which Hæckel has availed himself to the fullest extent.

It seems to us that Hæckel has needlessly increased the number of his families, sub-families, genera and sub-genera; this has been carried so far by him, that it appears almost like a satire on his own work, and we feel inclined to vary the question with which in a somewhat dramatic way he closes each one of his general chapters "und was ist bei den Anthomedusen eine bona species?" by another: and what is among Craspedota, a family, a sub-family, a genus or a sub-genus?

It is incredible that Hæckel with his great knowledge of Medusæ should so readily transfer merely from the drawings of others the young of species with which he is not familiar, either into genera with which they have absolutely nothing in common, or establish new genera for their reception. Hæckel also makes a number of imaginary corrections of others, evidently due to careless reading. I may mention among them the retaining of both the Laodiceidæ and Melicertidæ, by A. Agassiz and their relationship to the Polyorchidæ, and his complete success in restoring the former confusion existing among the Thaumantiadæ instead of clearing it up, his criticisms on Turris, on Obelia and Eucope which he himself disproves a few pages afterward by adopting the same method to separate those of his own genera!

It seems to have escaped Hæckel that already in 1863 A. Agassiz had called attention to the confusion existing in the family of Berenycidæ as adopted by Agassiz in his Contributions; to this Hæckel again refers in his system where he has figured and described a number of most beautiful forms of this interesting family. For some very exquisite and striking figures given by Hæckel I may call attention to the illustrations of the genera Sarsia, Dipurena, Tiara, Catablema; the figures of the Cladonemidæ, the Berenycidæ, Eucopidæ (Otorchis), the Geryonidæ and Narcomedusæ.

Among the Cladonemidæ the new genus Ctenaria is held by Hæckel to be the most closely allied of all Craspedota to the Ctenopheræ. At first sight this appears quite true, but if we

analyze more closely the combination of characters of the genus, none of which are new, as Hæckel himself states, we cannot help being convinced that Hæckel has exaggerated beyond measure the external resemblance, and that there is not in Ctenaria a single feature characteristic of the Ctenophoræ, while on the contrary every structural detail is met with in some other genus of the Tubularians (Anthomedusæ). The eight ribs of lasso cells of Ctenaria, similar to those of Ectopleura, certainly can not, in our present state of knowledge, be homologized in any way with the locomotive flappers of Ctenophoræ. Nor is the symmetrically forked chymiferous tube a ctenophoric feature; it is a character of other Cladonemidæ, and of Willia and other genera, where, however, the branching is not symmetrical. Nor is there anything in the genital organs, the stomach, or proboscis, which we do not find in other Tubularian genera, while we find nothing whatever like the above structures in any Ctenophore. And finally, if we imagine the pedunculated knobs of lasso cells, of Gemmaria and Pteronema to be scattered along the peduncles we have the tentacles of Ctenaria, and when they are reduced to knobs on the tentacle, we have the identical structure of many of the Sarsiadæ and the like. But as Hæckel's specimen was an alcoholic one it is as yet by no means clear that the tentacles of Ctenaria differed in any way from those of the other Cladonemidæ.

That the material still remaining for investigation is very great is well known to all who have had occasion to sail on tropical seas and to see the immense wealth of pelagic life float by; while Hæckel's work shows how much progress could be made in our knowledge of Acalephs by selecting a few properly placed stations where Medusæ could be studied advantageously. A. AG.

4. *List of Dredging Stations occupied by the United States Coast Survey Steamers "Corwin," "Bibb," "Hassler," and "Blake," from* 1867 *to* 1879*;* by BENJAMIN PIERCE and CARLILE P. PATTERSON, Superintendents of the Coast Survey. (Bulletin of the Museum of Comparative Zoology, at Harvard College, Cambridge, Mass. Vol. vi, No. 1), September, 1879.

5. *Ophiuridæ and Astrophytidæ of the Challenger Expedition;* by THEODORE LYMAN. Part II. (Bulletin of the Museum of Comparative Zoology at Harvard College, Cambridge, Mass., vol. vi, No. 2). December, 1879.

6. *The Cotton Worm;* Summary of its Natural History, with an account of its enemies and the best means of controlling it; being a report of progress of the work of the Commission; by CHAS. V. RILEY, M.A., Ph.D. Washington. 1880. (United States Entomological Commission. Bulletin No. 3.)

IV. Astronomy.

1. *Catalogue of the Library of the U. S. Naval Observatory.* Part I. *Astronomical Bibliography;* by Prof. E. S. Holden, 4to, 10 pp. Washington, 1879.—This is part of a proposed Catalogue of the very valuable library of the Naval Observatory. Professor Holden has, we understand, done excellent service in securing the completion and in arranging this library. The present bibliography is not strictly confined to the books now in the library, nor to the literature of observational Astronomy. It will be a very valuable help to those who have not, as well as to those who have, access thereto.

It seems to have been intended to cover the bibliography of "Astronomy, Geodesy, Optics and Mathematics." But just as only in exceptional cases it has gone beyond the limits of the Observatory Library, so has it only partially filled these three branches of science bordering on Astronomy. The author might have made his catalogue much more useful had he marked out a well defined field, however narrow, and then covered the whole of it.

The list of books here given lacks symmetry, and the selection is based upon some system we have not been able to discern, or else there was a want of that painstaking labor in its preparation which we had a right to expect in whatever comes from the Naval Observatory. That the list does not cover "Geodesy, Optics and Mathematics," will be seen by the simple mention of a few of the principal works a student in those branches must needs consult. Thus Todhunter's History of the *Theory of Attraction and the Figure of the Earth*, Glaisher's *Report to the British Association on Tables*, DeHaan's memoirs on *Logarithmic Tables*, Scudder's *Catalogue of Scientific Serials*, Dove's *Literatur der Optik*, The *Fortschritte der Physik*, The *Fortschritte der Mathematik*, are leading works in Geodesy, Optics and Mathematics, and yet are all left out. Though the work ought to have been better, it will be very useful as it is. H. A. N.

2. *Publications of the Cincinnati Observatory.* No. 5. 180 pp. 8vo. Cincinnati, 1879.—This number contains the results of the measurements made by the Director, Mr. Stone, and his assistants, Messrs. Howe and Egbert, between January, 1878, and September, 1879, upon 1054 double-stars. In accordance with a plan early adopted the stars measured have been principally in the southern hemisphere. The exceptions were mainly well known doubles measured for personal equation, and new doubles discovered by Mr. Burnham. Nearly 200 new doubles in all have been detected at the observatory.

3. *Catalogue of the mean declination of* 2018 *stars between* 0^h *to* 2^h *and* 12^h *to* 24^h *R. A., and* 10° *to* 70° *N. decl., for January* 1, 1875; by T. H. Safford. 4°, Washington, 1879.—This Catalogue was prepared under direction of Lieut. Wheeler of the U. S.

Engineer Department primarily for use in the geographical surveys west of the 100th meridian. The area in the heavens covered, is nearly one-fourth of the celestial sphere, being that part that is of use in field work. The aim has been to determine with the utmost accuracy attainable the declinations, and the annual precessions and proper motions in declination, of these stars, and incidentally the corresponding elements in R. A. The logs. of a' b' c' and d' are given for each star. For the region covered, this must for some time to come be a standard catalogue for the principal stars.

4. *Annals of the Astronomical Observatory of Harvard College.* Vol. xi, part II. Cambridge, 1879. By E. C. PICKERING, Director.—This part is a continuation of Photometric Observations made in 1877–9 principally with the large equatorial. The leading objects observed were the satellites of the various planets, a few of the asteroids, and about a hundred unequal double stars.

Assuming an albedo for the satellites equal to that of their primaries, and of the asteroids equal to that of Mars, Prof. Pickering arrives at the following diameters of some of the smaller members of the solar system in English miles, a result of general interest.

Phobos	5·57^m	Dione	542^m	Titania	586^m	Vesta	319^m
Deimos	4·85	Rhea	745	Oberon	544	Antiope	51
Mimas	292	Titan	1406	Sat. Nept.	2260	Brunhild	20
Enceladus	370	Hyperion	193	Pallas	167	Eva	14
Tethys	570	Japetus	486	Juno	94	Menippe	12

Japetus varies with his position in his orbit, which naturally leads to the conclusion that his time of rotation on his axis equals the time of revolution in his orbit, as is true for our moon. The diameters computed from the maximum, mean and minimum brilliancy are 574, 486, and 307 miles.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *The Smithsonian Institution; Journals of the Board of Regents, Reports of Committees, Statistics, etc.;* Edited by WILLIAM J. RHEES. 844 pp. 8vo. Washington, D. C.: 1879 (Smithsonian Miscellaneous Collections, 329).—This volume has been compiled in accordance with the instructions of the Board of Regents to the Secretary, to have prepared and to publish a history of the origin and progress of the Smithsonian Institution. It contains the Journal of Proceedings of the Board of Regents from its first meeting, September 13, 1846, to January 26, 1876, together with the reports of the Executive, Building, and Special Committees. It also includes eulogies on deceased members of the Board, and distinguished collaborators of the Institution; also, an account of the Bache Fund, the Tyndall trust, the Corcoran Gallery of Art, and various other similar matters.

2. *Erasmus Darwin;* by ERNST KRAUSE; translated from the German by W. S. Dallas, with a preliminary notice by CHARLES DARWIN. 216 pp. 8vo. New York, 1880. (D. Appleton and

Company).—The first half of this work contains a life of Dr. Darwin by his grandson, and the remainder is devoted to a discussion of his scientific works by Krause. The book will be read with appreciation by many, both in view of the interest which attaches to the general history of the Theory of Development, and because it shows how far the habits of thought of the older naturalist have descended to the grandson who has given his name to "Darwinism."

3. *Blowpipe Analysis;* by J. LANDAUER; authorized English Edition by JAMES TAYLOR and WILLIAM E. KAY, of Owens College, Manchester. 161 pp. 12mo. London, 1879. (Macmillan and Co.)—The original German work, of which this is an English translation, was written some four years since by Mr. J. Landauer. It follows the manual of Elderhorst in the arrangement, but differs from it and other works in that it is prepared from a chemical rather than an exclusively mineralogical point of view. The various means and methods of blowpipe analysis are fully and well described and arranged in convenient tables. It contains an extended description of the flame reactions of Bunsen, and, also, as introduced by Mr. Taylor, his own systematic course of analysis, and an account of the reactions with the aluminium plate of Ross. It will be found a useful and satisfactory hand-book by those desiring to become acquainted with the practical use of the blowpipe.

4. *American Journal of Mathematics, pure and applied.* Editor-in-chief, J. J. SYLVESTER; Associate Editor-in-charge, WILLIAM E. STORY; with the coöperation of SIMON NEWCOMB, H. A. NEWTON, H. A. ROWLAND. Published under the auspices of the Johns Hopkins University, Baltimore.—The following is the table of contents of the last number of this Journal:—

On the Geographical Problem of the Four Colors, (Plate II), by A. B. Kempe, B.A., (of London); Note on the Preceding Paper, by William E. Story; The Quaternion Formulæ for Quantification of Curves, Surfaces and Solids, and for Barycentres, by W. I. Stringham; On the Dynamics of a "Curved Ball," by Ormond Stone; Note on Determinants and Duadic Synthemes, by J. J. Sylvester; Tables of the Generating Functions and Groundforms for the Binary Quantics of the First Ten Orders, by J. J. Sylvester, assisted by F. Franklin; Note on the Projection of the General Locus of Space of four dimensions into space of three dimensions, by Thomas Craig; On the Motion of an Ellipsoid in a Fluid, by Thomas Craig; On Certain Ternary Cubic-Form Equations, chapter I, On the Resolution of Numbers into the sums or differences of Two Cubes, by J. J. Sylvester; A New Proof of the Theorem of Reciprocity, by Dr. Julius Petersen, (of Copenhagen); On a New Action of the Magnet on Electric Currents, by E. H. Hall. (This article is cited on pp. 200–205 of this Journal).

That a Journal of this character should be so well supported is honorable alike to American science and to the University under whose auspices it is published.

5. *Narrative of the Polaris.*—It is announced that Mr. Defrees, the Public Printer at Washington, will receive, until June first, orders for the new edition of the Narrative of the Polaris, at two dollars per copy. The money must be sent him with the order. This is the beautiful edition, of which extra copies

have been sold by authority of Congress at ten per cent above the cost of press work and paper.

6. *Bernhard von Cotta Fund.*—A request has been made to all the pupils and friends of Bernhard von Cotta, who died at Freiberg on September 14, 1879, to join in erecting a monument to his memory, and in establishing a fund which shall be called the "Bernhard von Cotta Stiftung." The memorial stone is to be erected at a suitable spot in Freiberg; the fund is intended for the assistance of indigent students at Freiberg, either to enable them to take part in geological excursions, or in more extended tours, or to facilitate their studies in other ways. The advantages arising from this fund are to be open to all worthy students, irrespective of nationality or creed.

Among the large number in America who have been friends or scholars of von Cotta there must be many who will take pleasure in responding to this appeal. The American members of the Committee are: Prof. G. J. Brush, New Haven, Ct.; Prof. F. Prime, Philadelphia; Prof. Raphael Pumpelly, Newport, R. I.; Dr. R. W. Raymond, New York.

7. *The Naturalist's Quarterly*, Vol. i, No. 1, January, 1880, Salem, Mass. (Naturalist Bureau).—A popular magazine devoted to Natural History in all its branches.

8. *A Geological Atlas of the United States and Canada.*—It is proposed by Professor C. H. Hitchcock, as the completion of a plan made some years since, to prepare a geological map of the United States. The support of those interested is called for in order to make it possible to carry through the undertaking. The responsibility of issuing the map has been accepted by Mr. Julius Bien, of New York (18 Park Place), on condition that a sufficient number of subscribers be obtained to cover the expense.

The base is the United States Centennial Map, revised and completed by order of Congress. It is to be 8x13 feet, and will be furnished with the geological colors, mounted on rollers, at $50, or in sixteen sheets at $45 per copy. An explanatory text will accompany the map.

9. *M. Dumas.* — "Nature" has published (Feb. 6) an extra number devoted entirely to an account of the life and work of M. Dumas, the eminent French chemist. The paper is prepared by Dr. Hofmann, of Berlin.

10. *Erratum.*—In the notice of Professor Cope's memoir, on page 155 of this volume, the number of species mentioned in the eighth line should be *thirty*-seven instead of seven.

Brain Work and Overwork; by Dr. H. C. Wood, Clinical Professor of Nervous Diseases in the University of Pennsylvania, etc. 126 pp., 12mo. Philadelphia, 1880. (Presley Blakiston.)

The Pathology of Mind; being the third edition of the second part of the "Physiology and Pathology of Mind," recast, enlarged and rewritten; by Henry Maudsley, M.D. 580 pp., 8vo. New York: 1880. (D. Appleton & Co.)

APPENDIX.

ART. XXXI. — *Principal Characters of American Jurassic Dinosaurs;* by Professor O. C. MARSH. Part III. With six plates.

IN the previous articles of this series, the writer has recorded the more important characters of several groups of Dinosaurs from the Jurassic deposits of the Rocky Mountain region.* In the present communication, some of the peculiar features in the structure of the *Stegosauria* are made known. This suborder proves to be one of the most specialized of the known Dinosaurs, and differs widely from the other groups.

Stegosaurus, Marsh, 1877.

The type genus of this group (*Stegosaurus*) may be taken as the representative of the suborder. Among the characters which at present distinguish this genus from the other known groups of Dinosaurs are the following:

(1) All the bones of the skeleton are solid.

(2) The femur is without a third trochanter.

(3) The crest on the outer condyle of the femur, which in Birds separates the heads of the tibia and fibula, is rudimentary or wanting.

(4) The tibia is firmly coössified with the proximal tarsals.

(5) The fibula has its larger extremity below.

Various other important characters of the present group, which are shared in part by some aberrant Dinosaurs, will be given below.

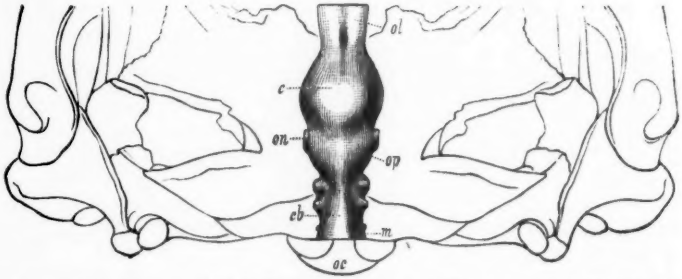
THE SKULL AND BRAIN.

The skull in the *Stegosauria*, so far as known, was remarkably small. In its main features it agreed more nearly with that of the genus *Hatteria*, from New Zealand, than with any other living reptile. The quadrates were fixed, and there was a quadrato-jugal arch. The jaws were short and massive.

* This Journal, xiv, 513; xv, 241; xvi, 411; xvii, 86; and xviii, 501.

Little has been known hitherto of the brain of Dinosaurs, but fortunately in one specimen of *Stegosaurus* the brain-case is well preserved, and apparently without distortion. Figures 1 and 2 of Plate VI show the form and general characters of this brain-cavity. The brain of this reptile was much elongated, and its most striking features were the large size of the optic lobes (*op*), and the small cerebral hemispheres (*c*). The latter had a transverse diameter only slightly in excess of the medulla. The cerebellum was quite small. The optic nerve (*on*) corresponded in size with the optic lobes. The olfactory lobes (*ol*) were of large size. As a whole, this brain was lacertilian rather than avian. A brain-cast of a young Alligator (figure 3) is given on the same plate for comparison. The contrast in the development of the cerebral region is marked, but in some other respects the correspondence is noteworthy.

In comparing the proportionate size of the brain of this living reptile with that of *Stegosaurus*, as given on Plate VII, the result proves of special interest. The absolute size of the two brain-casts is approximately as 1 to 10, while the bulk of the entire bodies, estimated from corresponding portions of each skeleton, was as 1 to 1000. It follows that the brain of *Stegosaurus* was only $\frac{1}{100}$ that of the Alligator, if the weight of the entire animal is brought into the comparison. If the cerebral regions of the two brains were alone compared, the contrast would be still more striking. This comparison, gives, of course, only approximate results, and some allowance should be made for the proportionally larger brain in small animals.



Outline of posterior part of skull and brain-cast of *Morosaurus grandis*, Marsh; superior view, one-fourth natural size; *ol.* olfactory lobes; *c.* cerebral hemispheres; *op.* optic lobes; *on.* optic nerve; *cb.* cerebellum; *m.* medulla; *oc.* occipital condyle.

The brain of *Stegosaurus ungulatus* is clearly of a lower type than that of *Morosaurus*, which, as the writer has shown, was several times smaller in diameter than the neural canal in its

own sacrum.* In the latter genus, the brain was proportionally shorter, and the cerebral region better developed, as shown in the cut above. The absolute size of this brain as compared with that of *Stegosaurus* is about 16 to 10, the brain of the Alligator figured being regarded as 1. Taking again the body of the Alligator as the unit, and *Stegosaurus* as 1000, that of *Morosaurus* would be about 1500. *Stegosaurus* had thus the smallest brain of any known land vertebrate. These facts agree fully with the general law of brain-growth, made out by the writer in extinct mammals and birds.

The Teeth.

The teeth of *Stegosaurus* are very numerous, and mostly cylindrical in form. Those from the maxillary figured on plate VI may be regarded as typical. The series represented in figure 4 consists of functional teeth in position, although separated from the jaw. The crowns are more or less compressed transversely, and are covered with thin enamel. The fangs are long and slender, and the pulp cavity is continued nearly or quite to the crown. The jaws contain but a single row of teeth in actual use. These are rapidly replaced as they wear out by a series of successional teeth, more numerous than hitherto observed in these reptiles. Figure 5, on Plate VI, represents a transverse section through the maxillary, immediately behind the fourth tooth. The latter is shown in place (1), and below it is a series of five immature teeth (2 to 6), in various stages of development, preparing to take its place. These successional teeth are lodged in a large cavity (*c*), which extends through the whole dental portion of the maxillary. The teeth in use were loosely implanted in separate sockets, and were readily displaced. The entire dental series evidently formed a very weak dentition, adapted to a herbivorous life.

The Vertebræ.

The vertebræ of *Stegosaurus* preserved all have the articular faces of their centra concave, although in some the depression is slight. They are all, moreover, without pneumatic or medullary cavities. On Plate VII, a selection from the vertebral series of one skeleton is given, which shows the principal forms. Figures 1 and 2 represent a median cervical. The other neck vertebræ have their centra of similar length, but the diameter increases from the axis to the last of the series. Some of the anterior cervicals have a small tubercle in the center of each end of the centra, a feature seen also in some of the caudals. All the cervicals supported short ribs.

* This Journal, vol. xvii, p. 87.

The dorsal vertebræ have their centra rather longer, and more or less compressed. The neural arch is especially elevated. The neural canal is much higher than wide. The head of the rib fits into a pit on the side of the neural arch. Figures 3 and 4, Plate VII, represent a posterior dorsal, with characteristic features. The ribs are massive, and strengthened by their form, which is **T** shaped in transverse section.

The sacral vertebræ are coössified, but their exact number in the present genus has not yet been fully determined.

The caudal vertebræ offer the greatest diversity, both in size and form. The anterior caudals are the largest in the whole vertebral series, and highly modified to support a portion of the massive dermal armour. The articular faces of their centra are nearly plane, and very rugose. The neural spine has an enormous development, and its summit is expanded into a bifurcate rugose head. These caudals are very short, and their neural spines nearly or quite in apposition above. These vertebræ have no distinct faces for chevrons. The transverse processes are expanded vertically, and their extremities curve downward. Further back, the same general characters are retained, but the centra are more deeply cupped, and the spines less massive. Figures 5 and 6, Plate VII, show a caudal vertebra from this region. The chevrons here have their articular ends separate, and rest upon two vertebræ. In the median caudals, the spine has greatly diminished in height, and the faces for chevrons are placed on prominent tubercles on the postero-inferior surface. The lower margin of the front articular face is sharp, and the chevrons do not meet it. In the more distal caudals (figures 7 and 8), the neural spine and zygapophyses are reduced to mere remnants, but the chevron facets remain distinct. These vertebræ, as well as those further back, have their centra much compressed. The caudal vertebræ are remarkably uniform in length throughout most of the series.

The Fore Limbs.

On Plate VIII, some of the bones of the scapular arch and anterior limbs of *Stegosaurus* are figured. The scapula and coracoid are of the true Dinosaurian type. The former has its upper portion rather short, and moderately expanded (figure 1). The coracoid was closely united to the scapula by cartilage. It is perforated by the usual foramen, which in some cases may become a notch.

The humerus (figure 2) is short and massive. It has a distinct head, and a strong radial crest. The shaft is constricted medially, and is without any medullary cavity. The ulna (figure 3) is also massive, and has a very large olecranal process. Its

distal end is comparatively small. The radius is smaller than the ulna. The fore limb, as a whole, was very powerful, and adapted to varied movements.

The Hind Limbs.

The pelvic arch of *Stegosaurus* is not complete in the specimens at present known, but its main characters agree with the Dinosaurian type. The acetabulum is formed by the ilium, ischium, and pubis. The last was apparently directed downward and forward. The ischium is shown on Plate IX, figure 1. It has a large head for union with the post-acetabular process of the ilium, and a thin extended vertical margin where it joins the pubis. At its distal end, it was united with its fellow by cartilage.

The femur of *Stegosaurus* (Plate IX, figure 2) is by far the largest bone in the skeleton. It is remarkably long and slender. There is no distinct head, and the great trochanter is nearly or quite obsolete. The shaft is of nearly uniform width, and very straight. There is no evidence of a third trochanter. The distal end of the femur is peculiar in having very flat condyles, with only a shallow depression between them. The external one has only a rudiment of the ridge which passes between the heads of the tibia and fibula, and is so characteristic of true Dinosaurs and Birds.

The tibia (figure 3) is very much shorter than the femur. Its superior end is unusually flat, indicating that it met the flat condyles of the femur so as to bring the two bones at times nearly or quite into the same line. The shaft of the tibia is constricted medially, leaving a wide space between it and the fibula. The distal end of the tibia is blended entirely with the convex astragalus, so as to strongly resemble the corresponding part in Birds.

The fibula (figure 3) is slender, and has its smaller end above. This extremity is applied closely to the head of the tibia by a rugose suture, so as readily to unite with it. Its upper articular surface is nearly or quite on a level with that of the tibia. The distal end of the fibula is expanded, and in the specimen figured is firmly coössified with the calcaneum. The two coalesce with the tibia and astragalus, and form a smooth convex articulation for the distal tarsals. The latter are distinct. The posterior limbs were more than twice as long as those in front.

The bones of the feet of *Stegosaurus* have not yet been fully identified, although a number have been found. In figure 4, Plate IX, a metapodial bone is shown, and in figure 4, Plate VIII, are views of a very characteristic terminal phalanx.

Dermal Spines and Plates.

The most remarkable feature about *Stegosaurus* is the series of ossifications which formed its offensive and defensive armour. These consist of numerous spines, some of great size and power, and many bony plates, of various sizes and shapes, well fitted for protecting the animal against assaults. Some of these plates are a meter, or more than three feet, in diameter.

The spines were of different forms, and varied much in size. On Plate X, four of these are represented. All of those preserved are unsymmetrical, and most of them are in pairs. One of the largest is shown in figure 1, which gives the more usual form and proportions. This specimen is over two feet (630 mm) in length, and its fellow is of the same size.

This spine has a rugose oblique base, and its sides are marked by vascular impressions and grooves similar to those on the bony horn-cores of ungulate mammals. It was evidently covered by a horny substance, and in life formed a most powerful weapon. The spinous appendage represented in figure 2 of the same plate was very similar in form and proportions, but of smaller size. It agrees closely with its mate, found not far from it. Nine different spines of this character were recovered with this same skeleton, and others may have been lost.

Figure 3 represents a different kind of spine. This also is obliquely truncated at the base, and thus is unsymmetrical, but its fellow has not been discovered. Its sides are flat and covered with vascular markings. There is a distinct ridge near the base, showing the depth this spine was inserted in the flesh. A smaller spine of the same general character was found near it. The small tubercular bone, shown in figure 1, Plate X, is very similar to the base of a spine-core, with the blade aborted.

The position these various spines occupied in life is uncertain, as none of them were found in place with portions of the skeleton fitted to support them. A spine somewhat similar to that in figure 2 was found with the skeleton of *Omosaurus*, in England, and regarded by Owen as a carpal appendage.* *Stegosaurus* may have been so provided, but the number and variety of the spines found with one skeleton indicate that various other parts were equally well armed. There are no indications of the attachment of spines to the tarsal region.

The dermal plates which protected the same animal were much more numerous than the spines. Some of them were so large and peculiar that their position is indicated by the structure of the anterior caudal vertebræ, whose enormous neural spines were especially adapted to support them.

* Palæontographical Society, 1875.

The plate represented on Plate XI, figure 2, was perhaps a dermo-neural spine, which stood erect over the caudal vertebræ. This would imply a deep compressed tail, and of this there are various indications. Several other plates found near the caudals probably occupied a similar position.

The largest plates discovered are similar to the one represented in figure 3. These are unsymmetrical, and their surfaces indicate that their position was on the back, arranged on each side of the medial line. There may have been several of these rows. Some of the smaller plates were discoidal in form, and quite thin. That shown in figure 1, is one of the smallest recovered. With such protection as the plates and spines together afforded, *Stegosaurus* was doubtless more than a match for his larger brained cotemporaries.

In considering the affinities of *Stegosaurus*, it would appear that the nearest known ally was *Omosaurus*. The fore limb, dorsal vertebræ, and one dermal spine are similar. The caudal vertebræ, however, are different, and there is no evidence that the latter genus was provided with plates, or that the skull and teeth were at all like those of *Stegosaurus*. They both may prove to belong in the same sub-order, and perhaps in the same family, *Stegosauridæ*.

The two known species of *Stegosaurus* were about thirty feet in length. They were herbivorous, and probably more or less aquatic in habit. It is possible that the difference between them was only sexual, as spines were found with only one.

The great disproportion in length between the fore and hind limbs, greater probably than in any known Dinosaur, would imply that *Stegosaurus* was more or less bipedal in its movements on land. The very short, powerful fore limbs, admitting of free motion, may have been well armed with spines, and thus used most effectively in defence. The back was evidently armed, as well as protected. When alive, *Stegosaurus* must have presented by far the strangest appearance of all the Dinosaurs yet discovered.

The remains of the animals here described are all from the Atlantosaurus beds of the Upper Jurassic, in Colorado and Wyoming. In bringing them to light, Messrs. Arthur Lakes, W. H. Reed, and S. W. Williston have rendered an important service to science.

Yale College, New Haven, Feb. 18, 1880.

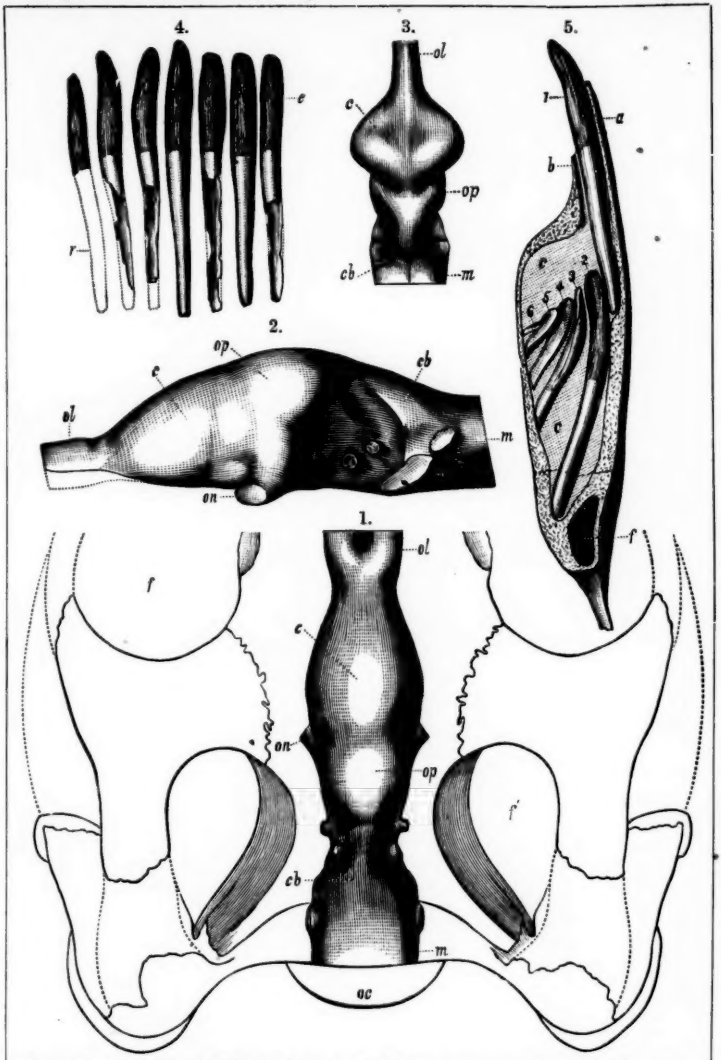


Figure 1.—Outline of skull and brain-cast of *Stegosaurus ungulatus*. Marsh; seen from above. one-half natural size; *ol*, olfactory lobes; *c*, cerebral hemispheres; *op*, optic lobes; *on*, optic nerve; *cb*, cerebellum; *m*, medulla; *f*, orbital cavity; *f'* temporal fossa; *oc*, occipital condyle.

Figure 2.—Same brain-cast; side view, one-half natural size.

Figure 3.—Brain-cast of young Alligator; three-fourths natural size.

Figure 4.—Maxillary teeth of *Stegosaurus armatus*, Marsh; side view, one-half natural size; *e*, enamel; *r*, root.

Figure 5.—Section of maxillary of *Stegosaurus armatus;* showing functional tooth in position, and five successional teeth in dental cavity; *a*, outer wall; *b*, inner wall; *c*, cavity; *f*, foramen.

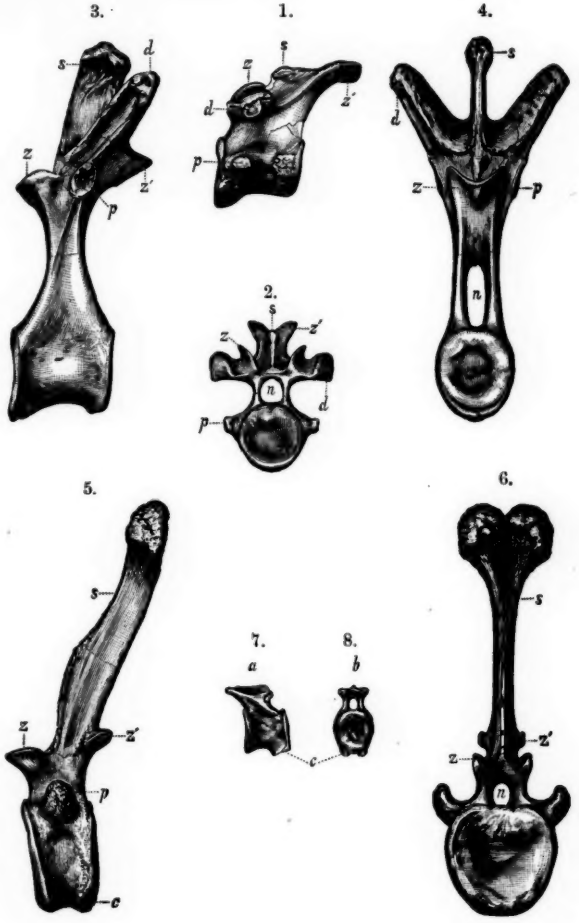


Figure 1.—Cervical vertebra of *Stegosaurus ungulatus*, Marsh; side view; *d*, diapophysis; *p*, parapophysis; *s*, neural spine; *z*, anterior zygapophysis; *z'*, posterior zygapophysis; *n*, neural canal.
Figure 2.—Same vertebra; front view.
Figure 3.—Dorsal vertebra of same series; side view; letters as above.
Figure 4.—Same vertebra; front view.
Figure 5.—Anterior caudal vertebra of same series; side view; *c*, face for chevron.
Figure 6.—Same vertebra; front view.
Figure 7.—Distal caudal of same series; side view.
Figure 8.—Same vertebra; front view.

All the figures are one-eighth natural size.

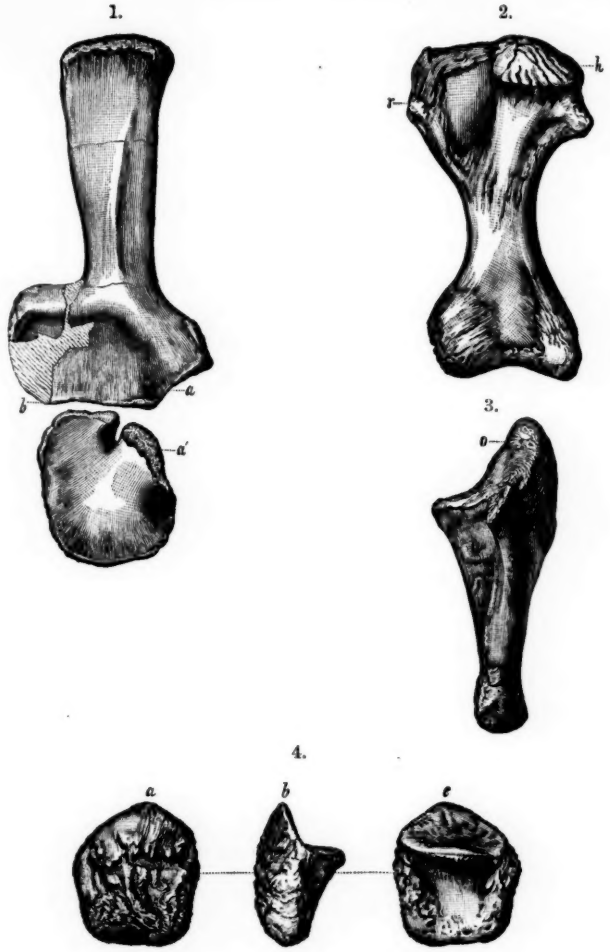


Figure 1.—Left scapula and coracoid of *Stegosaurus ungulatus*, Marsh; side view, one-twelfth natural size; *a*, scapular face of glenoid cavity; *a'*, coracoidean part of same; *b*, surface for union with coracoid.

Figure 2.—Humerus of *Stegosaurus ungulatus;* front view, one-twelfth natural size; *h*, head; *r*, radial crest.

Figure 3.—Ulna of same; side view, one-twelfth natural size; *o*, olecranal process.

Figure 4.—Terminal phalanx of same species; one-fourth natural size; *a*, front view; *b*, side view; *c*, posterior view.

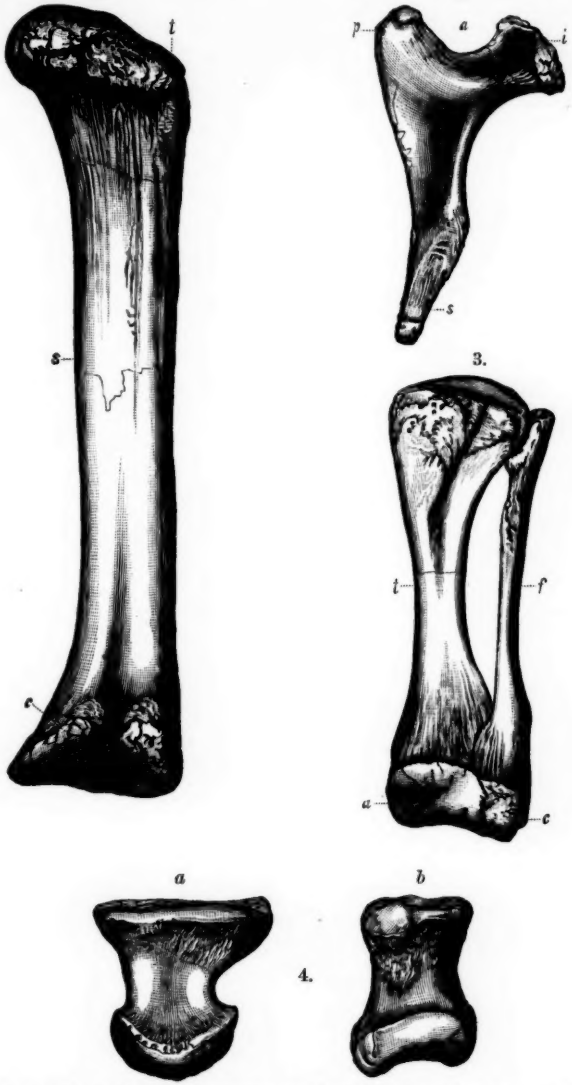
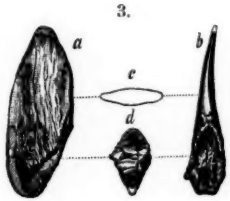


Figure 1.—Left ischium of *Stegosaurus ungulatus*, Marsh; side view, one-twelfth natural size; *a*, acetabulum; *i*, face for union with ilium; *p*, margin joining pubis; *s*, symphysis.

Figure 2.—Left femur of *Stegosaurus ungulatus;* front view, one-twelfth natural size; *t*, position of great trochanter; *s*, shaft, showing absence of third trochanter; *c*, inner condyle.

Figure 3.—Tibia and fibula of same limb; front view, same size; *a*, astragalus; *c*, calcaneum.

Figure 4.—Metapodial bone of same animal; one-fourth natural size; *a*, side view; *b*, front view.



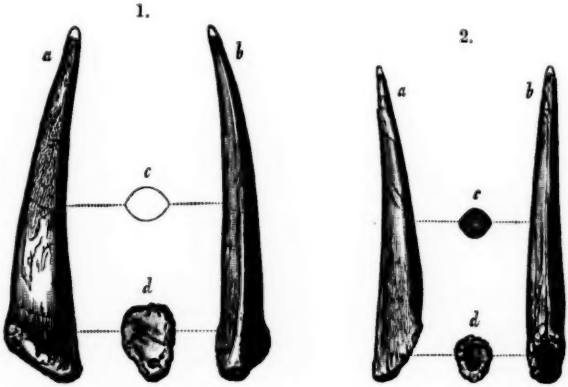


Figure 1.—Dermal spine of *Stegosaurus ungulatus*, Marsh; *a*, side view; *b*, front view; *c*, section; *d*, inferior view of base.

Figure 2.—Smaller dermal spine of same individual; *b*, posterior view; other letters as above.

Figure 3.—Flat dermal spine of same; letters as in figure 2.

Figure 4.—Tubercular spine of same species; *a*, superior view; *b*, inferior view; *c*, fore and aft view.

All the figures are one-twelfth natural size.

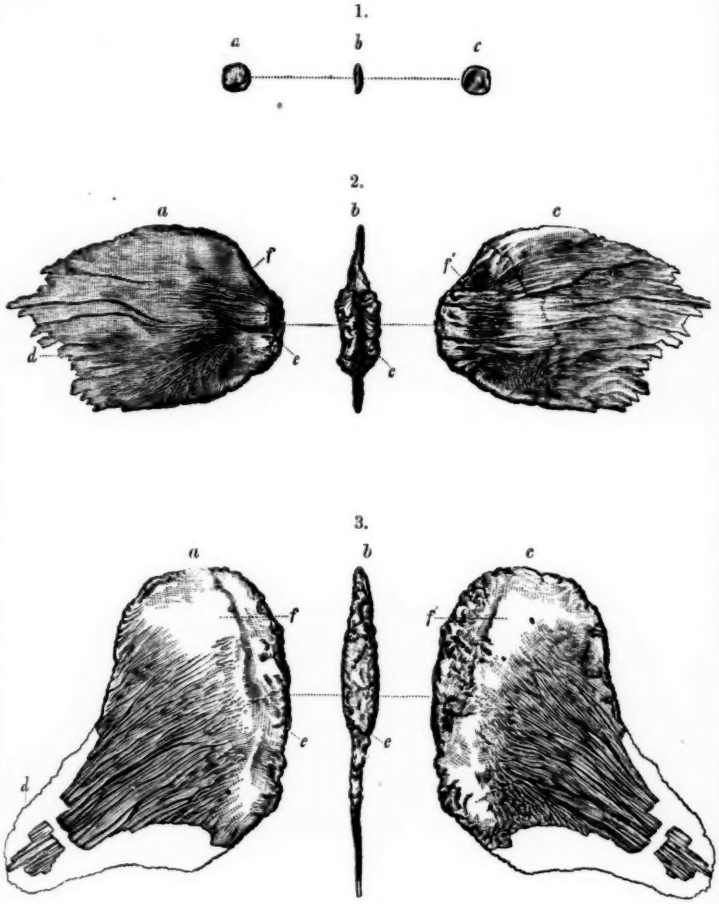


Figure 1.—Dermal plate of *Stegosaurus ungulatus*, Marsh; *a*, superior view; *b*, side view; *c*, inferior view.

Figure 2.—Dermal plate of same animal; *a*, side view; *b*, end view of base; *c*, view of opposite side; *d*, thin margin; *e*, rugose base; *f*, and *f'*, surface marked by vascular grooves.

Figure 3.—Dermal plate of same animal; *a*, superior surface; *b*, thick basal margin; *c*, inferior surface; other letters as in last figure.

All the figures are one-twelfth natural size.